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CONTENTS

The Barbour Lathrop Bamboo Grove, by David Fairchild 243

Biology and Government, by O. F. Cook and R. C Cook 250

Eugenics, The War Instinct and Democracy, by A. T. Winters 254

The Founders of the Art of Breeding—IV, by H. F. Roberts 257

A Commercially New Blueberry, by G. E. Murrell 271

The Dramatic Careers of Two Plantsmen, by David Fairchild 276

An Improved Strain of Wisconsin Tobacco, by James Johnson 279

Racial Origins and the Honors of War, by F. A. Woods 287

NOTE

Instead of raising the price of membership, and in order to meet the requirements of the War Industries Board, and also because of the drafting of the whole Editorial Staff the July, August and September numbers have been dropped out for 1918. Vol. IX contains only 8 instead of 12 numbers.

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Date of issue of this number, August 15, 1919.

THE ORIENTAL TIMBER BAMBOO AS IT GROWS IN THE UNITED STATES

A view in the edge of the Barbour Lathrop Bamboo Grove near Savannah, Georgia. It is not generally k nown that this "giant" bamboo of China and Japan is successful throughout a wide area in the southern part of the United States. In addition to its ornamental value, the bamboo is one of the most useful of plants. (Frontispiece.)

THE BARBOUR LATHROP BAMBOO GROVE

DAVID FAIRCHILD

WHILE traveling in search of plants in Japan in 1902, Mr. Barbour Lathrop and I became interested in the possibilities of the cultivation of the Japanese bamboo in America. Acting under the stimulus of his enthusiasm, I wrote a bulletin on the subject for the Office of Foreign Seed and Plant Introduction.[1] Seventeen years have passed since then, bringing added experience to us in regard to this subject—experience which has resulted in the firmest conviction that from the Carolinas to the Gulf, wherever the soil conditions will permit, groves of the Japanese bamboo can be grown that will be as large and as beautiful as any in Japan.

We are now growing in this country both the timber and the edible bamboo, with stems as tall and with plumes of foliage as feathery as anything to be seen in Japan or China, and there is no reason why, at thousands of places, we should not start small groves of this plant which the Orientals have for thousands of years considered the most useful plant in the world.

It is a strange thing to consider that on one side of the Pacific there are civilizations comprising hundreds of millions of people who are so dependent upon the bamboo that they simply cannot imagine an existence without it, whereas, on the other shore, a hundred million people live whose main contact with the plant is through its use as a fishing pole.

Now that the experiments begun by the Government in 1902 and the many individual trials of the plant, which were begun earlier, have shown how easily the bamboo can be grown, the task is before us of instructing the American farmer in the uses and value of this remarkable plant. In order to do this, material in considerable quantities will be required, and it is a most fortunate circumstance that at this critical point in the development of the industry in this country. Mr. Lathrop, who has fathered its introduction, should not only save from probable destruction the most remarkable grove of bamboo in eastern North America, but that he should place it in the custody of the Department of Agriculture for ninety-nine years, in order that the timber which it produces and the thousands of young plants which can be taken from it may be placed by the Government in the hands of the manufacturers on the one hand and the farmers on the other to experiment with.

The grove which Mr. Lathrop has recently acquired is located on the Ogeechee Road, 14 miles from Savannah, Georgia. The Dixie Highway passes by it on that stretch which connects Savannah and Brunswick, and every lover of plants would find that the experience of a few minutes in this unique grove rivals any of the other new experiences of a motor trip through the south, for a bamboo grove is strangely different—unusually so—from any other growth of trees in the world.

The dense, deciduous tropical forests of Java and Sumatra, the evergreen fir and spruce forests of Canada, the eucalyptus-covered plains of Australia, the rainy region jungles of Brazil, the date palm groves of Egypt, or the fern tree forests of Hawaii are all different from each other, but they have this in common—the trees have trunks, and

[1] Fairchild, David G., Japanese Bamboos and Their Introduction into America. Bul. 43, Bureau of Plant Industry, Office of Foreign Seed and Plant Introduction, 1903.

THE BEGINNING OF THE BARBOUR LATHROP BAMBOO GROVE

When this photograph was taken, about 1900, the bamboo plants were just tall enough to be seen over the roof of the house. They were then about 10 years old and had all grown from a single plant which Mrs. H. J. Miller, of Savannah, planted in the back-yard near the pump. She got the original rhizomes from a small planting near Burroughs, Ga., which was set out by Mr. Andrias E. Moynelo, who brought a plant from "India." Compare this with the following photograph. (Fig. 1.)

THE SAME GROVE NINETEEN YEARS LATER

From a single clump planted by Mrs. Miller have come all the tall feathery shoots which now tower far above the house, shade it, and protect it from the wind. They form what is without doubt one of the most attractively shaded dooryards in America for children to play in. Compare this with the photograph taken from the same point 19 years previously (the old store shown in the previous photograph has been removed). (Fig. 2.)

above them are the spreading branches or branch-like leaves. They agree in a general way with one's conventional idea of a tree. A bamboo grove, however, is a different thing entirely. It is a giant grass. Like a grass it forms a perfect mat or sod of roots, which completely fill the ground, and like a grass this sod in the spring sends up everywhere new young shoots which, instead of starting out as miniature forms of the trees they are to become—growing slowly larger with each passing year—come up full sized through the ground, and they come with a rush, raising the soil and cracking it as a mole does. Often 4 inches in diameter, these shoot skyward at the rate of a foot a day, until they stand towering above you 50 or 60 feet in the air. Put your hand on one of these new shoots, green as an asparagus shoot. If you shake it, the top will snap off and fall down on your head, so soft and brittle does it remain until it has reached its full height and spread out its delicate branches of thin green leaves.

On all sides, so close that you can just walk between them, rise these giant green canes, branchless for 20 or 30 feet. At every foot or two there occurs a joint, like the joints on a tall grass, and, like the grass, the lengths between the joints—the internodes—are hollow. Each encloses a dead air space, so completely shutting it in that, if you heat the air by throwing a cane into the fire, it will explode with a loud report.

The charm of a bamboo grove lies in the friendly mystery of its shade, with the green sunlight flickering through the thin plumes of leaves on to the soft mat of yellow dead leaves below. You wander through such a grove, feeling that you have never seen anything like it before, and the quiet, fairy-like charm of it remains with you long after you have gone away.

As a landscape unit alone, therefore, a grove of bamboos is worthy of the widest popularity, and parks and open places through the south should plant them. I can imagine no more wonderful a place for little children to play in than a bamboo grove.

But there are other and more practical reasons for the widest possible distribution of the bamboo throughout the south. To begin with, its young shoots furnish one of the most delicious of early vegetables. In Japan and China it plays a loading rôle among spring vegetables, and one finds it on the tables of the well-to-do Americans and Europeans everywhere, as well as on the Japanese menus. Properly cooked, it has a freshness and flavor which always reminds me remotely of sweet corn, but its texture is firmer than almost any other vegetable I am familiar with. It comes into the market in April and, being a good shipepr, it should quickly win its proper place on the American menu, where early vegetables are always a desideratum. In Japan, the edible bamboo growers make good money out of the sale of the shoots—in fact, bamboo culture ranks as one of the most profitable vegetable industries of the country. I have eaten American-grown bamboo shoots, and those of my friends who have done so have, without exception, liked them extremely well.

To speak of the timber uses of the bamboo is a good deal like speaking of the timber uses of the pine, except that the qualities of the two are entirely distinct. The peculiar properties of the bamboo come from the fact that the trunks are composed of short, hollow cylinders, making them the lightest timbers for their strength of any known. The fibers run from end to end, making it possible to split a bamboo pole into strands or strips more easily than any other wood. These fibers are among the strongest, most elastic known, and it is the bewilderment of Occidentals visiting Japan to see how many are the uses of this peculiar timber.

For ladders, their lightness and strength render the poles remarkably well adapted; there are no fruit-picking ladders which compare with them. For fishing poles, the small canes are

THE DARK MYSTERY OF THE BAMBOO GROVE

Had Maxfield Parrish lived among the bamboo forests of Japan or China, he would have found in them the themes for many of his paintings which appeal to our imaginations by their sense of mystery. In this photograph the bamboos are nearly as tall as the pecan trees standing in their midst. (Fig. 3.)

A DOORYARD SHADED BY BAMBOOS

Oriental babies have played for fifty centuries in such cool, shaded, leaf-carpeted spots as this, but to American children they are practically unknown. In addition to the pleasure and fascination of such a grove, there is an education in the uses to which the twigs, hollow stems, and leaf-sheaths can be put by children. (Fig. 4.)

imported by the millions from Japan and scattered by jobbers all over the country so that every boy can have one, and the split bamboo fishing poles, of which we use nearly $5,000,000 worth every year, are made from the outer layers of wood of the best canes. What baskets can compare in their variety and delicacy of construction with the bamboo baskets of Japan! For watering pipes for small gardens they will have the same use in America that they have in Japan, the joints being broken out with a long iron rod. Our best flower stakes are bamboo. Bamboo bean poles cannot be excelled, and for light arbors, fences, trellises, vine stakes, penholders, pipe stems, kindling wood, spraying nozzle holders, and many kinds of tool handles, they find a wide use in the Orient.

Anyone who has used a bamboo handled broom will appreciate its lightness and the smooth finish of its surface, while the split strands of bamboo are so stiff and elastic that they have been used most successfully in broom making—they may even compete with the broomcorn straw for broom manufacture.

While these are a few of the obvious uses which will probably be first investigated by Americans, it is inconceivable that the ingenuity of the American will not find new uses for so unique a raw product as the bamboo, and the Barbour Lathrop grove will play a most important rôle in the education of the people of this continent in the uses and beauty of this remarkable plant.

So far as can be ascertained, the original plants from which this grove sprang were brought into America by Mr. Andreas E. Moynelo, a Cuban by birth, who was at one time one of the largest rice planters on the coast of Georgia. Mr. Moynelo traveled in the Orient and is said to have brought the plant back with him from the East Indies, but from what particular region is unknown. The importation took place probably in the late eighties. The first planting was made several miles distant from this grove, at the village of Burroughs, and from it, probably within a few years of the time of its first planting, a small plant was taken by Mrs. H. J. Miller and set out near the well back of the house. This was in 1890, so that the grove today is twenty-nine years old. Although called an East Indian species, it appears to be very closely related to the best of the Japanese timber bamboos (*Phyllostachys bambusoides*), from which it seems to differ in no reliable character. It appears highly probable that Mr. Moynelo got his plants in Japan or China.

An Exposition of "The New Psychology"

THE CHILD'S UNCONSCIOUS MIND, by Wilfrid Lay, Ph.D. Pp. 329. Price, $2. New York City: Dodd, Mead & Co., 1919.

The type of analytical psychology which is usually associated with the name of Freud serves as a useful corrective to exaggerated claims of inheritance of mental traits; but some of its exponents show little realization of the facts of heredity. Thus Dr. Lay, whose ill-written book contains a good deal of interesting material, recalls that "the actual nervous constitution, which is determined for the child before the hour of birth, is the inheritance of an infinite number of ancestors, all of whom contribute an approximately equal part." It would be difficult to frame a more misleading sentence on the subject. Again, he "is impressed with the tremendous unity of nature, everything except man apparently completely fulfilling its appointed function all the time without interruption." The apparently imperfect things in nature are "as perfect as their environment allows them to be;" and "is not the most bestial human the best product that his circumstances could make of him?" As Dr. Lay has a good many suggestive ideas on education, it is a pity that his book could not be marked by a sounder biological point of view.

BIOLOGY AND GOVERNMENT

Further Discussion of Alleyne Ireland's Article on Democracy and the Accepted Facts of Heredity.

O. F. Cook and Robert Carter Cook

DR. WOODS has asked for biological consideration of questions raised by Mr. Alleyne Ireland in an article entitled: "Democracy and the Accepted Facts of Heredity," in the December number of this journal. To see that theories of government need to square with biological facts is an advanced position in political philosophy. Biologists may not accept the applications proposed by Mr. Ireland, but there can be no question of the need of bringing the two fields of thought into closer relations.

That acquired or environmental characters are not inherited, that mental and moral traits are inherited, that selective or assortive mating tends in nature to preserve adaptive or advantageous characters, that progressive evolutionary changes appear as individual variations, not as mass transformations, are statements that most biologists accept, but they do not seem to afford a basis for Mr. Ireland's reactionary theory of government. The question of breeding families or classes of great, ideal rulers has a flavor at once medieval and utopian, but human interests are many-sided and need to be viewed from various angles.

The proposition that assortive mating separates the population into distinct superior and inferior groups would need to be supported by definite evidence.[1] Wild species do not divide or become bimodal in relation to characters of adaptation, probably because the non-adaptive extreme is eliminated by natural selection. The human species may not become bimodal for the opposite reason, that civilization tends to eliminate the more capable and to preserve the inferior or the mediocre, the meek who inherit the earth after the more aggressive have destroyed each other or exhausted themselves in the struggle.

The main difficulty in applying general biological facts to human interests is that people in civilized countries are no longer engaged in a struggle for existence, but endangered rather by superfluity. The primitive necessities of food, clothing and shelter are now placed within reach of all, through applications of science to agriculture and other arts. How to protect ourselves against greed, luxury and urban degeneration is the biological or eugenic problem of civilized countries. The use of alcohol is only one artificial habit that is clearly prejudicial to the race under the conditions of our present civilization. The entire system of sedentary urban existence, of education, commerce and industry, undoubtedly has a sterilizing tendency that is exerted especially upon the superior stocks. The continual drafting of the more capable elements of the population from the country to the city is a persistent and dangerous form of adverse selection, as explained in an article on "Eugenics and Agriculture," published in June, 1916, in this journal. Hence, instead of becoming bimodal or tending to separate into superior and inferior groups, as Ireland imagines, our tendency is to

[1] Mr. Ireland has here doubtless drawn upon some unpublished evidence which I have been collecting for a number of years showing that the lower elements of society are being more and more removed from the upper, presumably by the action of assortative mating. The reasons why this should take place on *a priori* grounds are extensively discussed in the final chapter of my "Influence of Monarchs," 1913.—Acting Editor.

restrict ourselves further and further toward mediocrity and inferiority.

That useful talents may appear in "lower levels of society," as Professor Conklin points out in the May number, may mean that our system fails to place good stocks under favorable conditions, or that our judgment of conditions is at fault. The need is to give special ability or usefulness a selective value, to preserve and increase the family stock, but our system works generally in the opposite direction of using up and exterminating talent as rapidly as possible. Thus there are biological problems that need to be studied from the standpoint of politics, as well as questions of government that need biological answers.

Mr. Ireland's "broadest generalization," from his investigation, "that the best governed countries were those in which the mass of the people had the least control over the administration of public affairs," indicates Germany as the shining example, although not so stated. Germany, at least, will do very well to show the advantages and the disadvantages of such a government, for certainly there was no approach to effective control of public affairs by the people. Germany was a hereditary monarchy, so that the government was in the hands of one who had not only special training as a ruler but any hereditary advantages of descent from a line of kings. More than that, the fabric of government in Germany had woven in it a fairly strict caste system, so that not only the ruler, but the governing class as well, was not selected by mere numbers. At any rate, the numbers were so well balanced that they had no disturbing effect. The Krupp works employed about 18,000 men in peace times, and the directors of the firm had 18,000 votes allowed them in the elections; the "blind God of Numbers" was propitiated.

Superficially, at least, the results attained by this system seemed all that could be desired. German cities were cleaner than English or American cities. The visitor to Germany did not see the docks of Hamburg lined with human scarecrows, or little children going barefoot in the snow, as in the streets of Liverpool. Disheveled slums like those of London or New York did not exist. The tenement population of Berlin lived in improved modern buildings, along wide, clean pavements. There were no hungry beggars or aimless people wandering about in rags. Irresponsible indigence was as strictly forbidden as other misdemeanors. Everybody had something to do, and somebody to keep him at it. Only the Kaiser could do as he pleased.

Yet all these busy Germans were discontented. They knew that their activities, and even their prosperity, were artificial and that they were intended to serve the purposes of the military caste, as pigs are fattened for the slaughter. Indeed, they often referred to themselves and to their children as "cannon-fodder," foreseeing their fate as inevitable. With a less efficient government the Germans might have been less industrious, less clean, less orderly, and less aggressive. Internal dissensions and party struggles might have been carried farther and interfered with preparations for attacking other nations. Probably a democratic Germany will not be so much governed, and devotees of efficiency may weep for the good old days of Hohenzollern power.

No doubt autocracies are the "strongest" governments, or can be made very strong in the hands of a practical and prudent clan like the Hohenzollerns. The trouble with the most benevolent autocracy, whether it be a monarchy, an oligarchy, or any system in which a single class has full authority over other classes, is that it does not stay benevolent, or humane, or practical, or prudent, but generates destructive abuses of power. Courts of monarchs are seldom frequented by wise men, but have ever been the haunts of parasitic adventurers. If monarchs seek advice from anybody, it is from military men to strengthen their armies or from financiers to increase their revenues. Dominance of a

military or priestly caste is a familiar feature of strong, centralized governments. This is not only because soldiers and priests are found useful in maintaining power, but because separate tastes tend to seek their own interests and readily serve the purposes of parasitic governments in maintaining forced control of the people.

Mr. Ireland reflects on the fact that there have been only a few hundred men recognized as really great among the thousands of millions who have lived on the earth, and that "we owe all the inspiration of religion, philosophy, music, art, and literature; all the benefactions of science, discovery, and invention to the genius of a few hundred individuals." The existence of such men certainly must not be left out of account in relation to the question of government. If it appeared that more great men were produced by centralized governments, the argument for monarchies would be strengthened on the biological side, but history shows that many of the conspicuously great men have lived and done their work in conflict with the monarchs of their time. Kings and princes do not wish to be overshadowed by any of their subjects. Neither are great individuals developed by governments that make the individual a cog in a machine, or that exalt the state in order to treat the existing order as sacred and suppress individual efforts for reform.

Germany has contributed several of the great individuals who have lived, but the great Germans appeared while the country was a fairly free though chaotic aggregation of small states. Since Germany became a very efficiently organized empire she has produced no conspicuously great or famous men. There are no Goethes, Schillers, or Humboldts in Germany today, and such personalities are not likely to develop under the kind of government Germany has had during the last fifty years.

To leave the government of a country entirely to a group of experts who could always be trusted to discharge their duties in the most competent and benevolent manner, and who were entirely incorruptible and free from dangerous ambitions, might be approved as an ideal plan, though without bringing us nearer to a practical realization. Time and again this ideal possibility has been put to the test, when governments have been taken in hand by those who had shown themselves best qualified for control. Many great men have served as kings, or as rulers of other kinds, and have organized their governments with the help of the most competent advisers, but nowhere in history has any family, clan or class been able to maintain itself permanently by capable leadership. Vanity, ambition, and greed are so general and so dominant, in human psychology, that every system of arbitrary power gravitates naturally toward oppression and parasitism. Eventually the system decays so far that revolution can put an end to it, or another party grasps the power, and the cycle is repeated.

However well an autocracy works for a time, the national interests run eventually into the narrow channels of arrogance, selfishness and stupidity of the ruling class. Forgetting that their authority was given them by man, they end by claiming supernatural power, or special relations with the Deity, beyond the reach of any restraining idea. Fear of God and regard for man alike disappear in their exalted self-assurance. The crux of the matter seems to be that the exercise of arbitrary power is essentially unjust, unsocial, immoral, and destructive to those who use it. "For somehow this malady attaches to tyranny, not to put confidence in its friends."

That a system should be strong and efficient, so that it can direct, dominate and project itself widely is the ideal of government inspired through the sense of power. Many are fascinated by the idea of one man or a small group of men seated on a pinnacle of authority, controlling the fate of millions. But we are now beginning to see that this sense of power is a primitive instinct, and is being outgrown. It is much stronger among savages or in barbarous coun-

tries than among civilized people. To know what should be done is the first qualification of constructive leadership. Fully intelligent, right-minded men no more desire to control than to be controlled. For those who are anxious to find their place in the world and to do their share of the work, external compulsion is not needed. Organization, plans, and directions that coördinate effort are understood and obeyed, but arbitrary, unnecessary orders are resented.

The sense of present-day humanity—that popular government is the best—is the product of the long experience that history records and is not in conflict with biology or with the scientific spirit. Freedom is the eldest daughter of Science, according to Thomas Jefferson, who saw very clearly that the human mind must be liberated in order to work out a higher destiny, and that this provision for progress must be a fundamental consideration in government. The Napoleonic theory that "governments are established to aid society to overcome the obstacles which impede its march" is less profound because it does not so clearly recognize the source of constructive progress in the scientific mind, and the need of excluding all forms of artificial domination and oppression of the mind, as well as the physical forms of injustice. Scientific men, even more than others, should appreciate the need of progress in government to meet changes of other kinds. Our experiment in democracy is very different from any that preceded it, in being aimed toward scientific government.

Mr. Ireland's theory of government does not appear progressive, but distinctly archaic. It is conceived, as he says, "on the level plain of routine," the object being "to direct the energy of the mass," a project like making all the clocks strike at the same instant, as in the traditional effort of another disillusioned specialist in government. Instead of looking forward to a world of capable, right-minded people, "wisdom and talent" are to be established in authority as a select class, to avoid a catastrophe that threatens the existing order through the advance of democracy.

But there would be more danger that placing a select class in control would insure its destruction in the usual way, instead of tending to preserve it. Responsible majorities are not dangerous, but irresponsible minorities, of parasites or fanatics. Where has democracy produced any such reactions as the French Revolution a century ago, or the Russian Revolution of today? What are the principles or theories that need to be applied in government, but cannot be presented and explained for the approval of a free electorate?

Not too much power in the hands of the people, but too little of active, constructive interest in problems of general welfare, even among intelligent citizens, is the most serious danger in popular government. People think of the government much as they think of an insurance company. They pay their tax assessments passively and assume that a mysterious system will attend properly to the public work. Our theories are democratic, but many of our habits are still feudal or monarchic. We are far enough from monarchy to think of the government as responsible to the people, yet not far enough for people to think of themselves as responsible for government. A special lesson on this point is now being had in Germany.

The general problem of government is to develop popular systems along lines of wider and more direct interest to the people, not to restrict interest or responsibility to a special governing class and revert to institutions of arbitrary power. The discussion of eugenics and other applications of biology no doubt will lend more constructive interest to political questions. Eventually there may be developed a truly biological conception of the race as a living organization, and of governments and other social institutions as means of conscious adaptation, to meet progressive needs in each generation.

EUGENICS, THE WAR INSTINCT AND DEMOCRACY

JOURNAL OF HEREDITY:

Certain articles that have appeared in the journal during the last five or six months have moved me to a certain amount of speculation, which I now put forward tentatively. They are merely the speculations of a layman.

In one number of the journal (which I have not at hand and cannot definitely refer to) was an article (by Woods, I believe) to the effect that war might be beneficial by aiding Nature in her problem of the survival of the fittest.[1] Does not war rather tend to take the fittest from each nation, and kill, or maim, or break them with disease, and in this way, rather than leave Nation A stronger than Nation B, leave B weaker than A, and both A and B, and hence the race, weaker than before? In the same number was an article by Popenoe which attempted to demonstrate that war is inevitable, because of an innate instinct in man for war, an hereditary instinct, an instinct which is therefore as ineradicable as one's heart or lungs. Is there not rather a desire for occupation, for a struggle for existence, which under favorable circumstances may be directed along industrial, scientific, and artistic lines, but which under unfavorable conditions may run to marauding and bloodshed? It is a notable fact, at any rate, that all of the wars in the history of civilization have been due either to the ambitions of some foolish monarch or to the commercial interests of those classes from which a monarch draws his support. Perhaps if these wars had not occurred, others would, due to the innate desire of which the author speaks; perhaps peace would have reigned; there is no telling. But supposing both or either of these theories to be correct, they seem to me, unless followed to their natural conclusion, to be rather dangerous doctrines to preach. There are four possibilities in regard to them.

1. War is neither beneficial nor inevitable, in which case we should do everything in our power to forestall the possibility of war.

2. War is beneficial but evitable. In this case one can only ask *how much* excessive war is beneficial, for certainly too much war means extinction. What scientist will draw the line between too much and enough? I scarcely think there is any. Then we take no more risks in evading war than in indulging in it—for how can we tell that we do not indulge too much? And war is a different thing to control, anyway—once it gets started there is no saying how far it will go. A well-directed system of executions would be much safer. So the same thing to do in this case, of course, would be to look to our present weal as we cannot look to our future and forestall any possibility of war.

3. War is detrimental but inevitable. If this be true we can only remove all superficial causes of war, and for the rest either resign ourselves or shake our fists at the Unknowable, as our natures or religions dictate.

4. War is beneficial and inevitable. In this event, war is a result of natural law, and, as a result of natural law, is beneficial. But it no less surely follows that any war that is not the result of natural law, but the result of super-

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[1] The article is by Alleyne Ireland in the November number, 1918. It concerns a quotation from "Is War Diminishing?" by Woods and Baltzly. The contention, however, was never made by these authors that war is eugenic. Woods pointed out some possible eugenic aspects of war that were ignored by partisan writers and by the then numerous pacifists. The disgenic aspects of war are also here considered, and the contention is made that further researches are necessary. —ACTING EDITOR.

ficial causes (the commercial interests of a few, the whims of a monarch) is not beneficial; for it goes without saying that the desire of a few obsessed mammon-worshipers does not represent the innate instinct of the mass. Only such war as is inevitable is beneficial, for again too much war will surely lead to extinction. It then follows that we should remove all superficial causes of war and only indulge in such wars as are dictated by nature, *i. e.*, by the desire of the people of a given nation *as shown by a popular vote*. By this method and by this method only can we evade interference with nature.

Hence it follows that whether neither, either, or both of the postulates that war is beneficial and that war is inevitable are true, our course of action should be the same—to forestall the possibility of war in every way.

The other bone I have to pick is Ireland's article in the December number: "Democracy and the Accepted Facts of Heredity." Ireland makes the point that some people by their intellectual heritage are more fit to govern than others, and that these individuals should be given something of a free rein in governing. If these people would govern for the good of their nation at large, and not for the good of themselves and their small cliques of supporters, this would be true enough. But although certain men have the intellectual ability to govern efficiently, I dare say that not one in a century has sufficient moral strength to govern justly if he is not held in check by the chances of dethronement. I do not hold up popular control as the panacea of all evils, but it is likely to exert a restraining influence on the ambitious. It is a historical fact, of course, in practically every country, that social and economic reforms (decent housing conditions, living wages, etc.) have not come from the governing classes, but from and because of the demands and unrest of the masses. And while I do not support any religious dogma, I do hold that every man has a right to a square deal. Moreover, I hold that war is, to all intents and purposes the offspring of a government by (and therefore for) the few. If war had only been declared after a popular vote had shown a people in favor of it, I venture to say that there would have been very few wars in history.[2] And I venture to say that the individual nation and the world at large would be no worse off if this custom prevailed. The German and Russian workers and peasants would scarcely have been sufficiently interested in the jealousies of their masters to have started the holocaust that has just terminated. As to Mr. Ireland's fear for the fate of the individual, such individuals as Napoleon, the late Kaiser, or Bismarck would doubtless feel themselves hampered by a democratic government. And it is unfortunately to Napoleonism and Bismarckism that brilliant minds run when given unlimited power. As far as the arts and sciences go, I think Mr. Ireland can safely put away his fears. When even such a radically popular government as the one at Moscow puts such a man as Lunacharsky at the head of its educational system it does not point to a disapproval of personal genius. Even Trotsky would scarcely advocate that pictures be painted by referendum or that esthetic criticism be put upon a communistic basis. Moreover, even though Mr. Ireland may not consider the ten million odd of the *hoi polloi* killed in the recent war—precipitated by autocratic governments—to be important, still he must admit by his own argument that the death of such men as Gaudier-Brzeska, Rupert Brooke, Peguy, Frakl, etc., were a genuine and irremediable loss to civilization, provided, of course, that he has ever heard of these men. And no one knows how many potential Shakespeares and Newtons there were among the nineteen and twenty-year-olds that were slaughtered and broken.

The chief crime of democracy would seem, then, to be a relative inefficiency in the handling of public affairs, of

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[2] This should be decided by a systematic appeal to the facts of history. Some facts as yet unpublished indicate that democracies are quite as prone to war as are autocracies.

autocracy, selfishness, which results in war, slaughter, disease, and the degeneration of mankind.

There is also the esthetic argument for democracy which I shall at present refrain from expounding, as Mr. Ireland would doubtless consider it a sort of fourth-dimension vaporing. It is, notwithstanding, responsible for the opinions of a considerable portion of the world's population.

I repeat, however, that I offer these opinions not as the opinions of an expert, but as the opinions of a layman. It is only because I suspect that a great many other laymen may hold them that I trouble you with them. But this being the case, I think it would be worth while for your experts to set me and my comrades in ignorance right on these matters if we are wrong. For such, I take it, is the social function of experts.

Very truly yours,

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A FACTOR INFLUENCING THE SEX-RATIO

During the latter part of the nineteenth century it was generally believed that sex in man and in various animals is determined mainly by the amount of nourishment that the embryos receive; well-nourished embryos were supposed to become females; those that were poorly nourished were assumed to develop into males. A considerable amount of evidence in favor of this view was collected by Düsing ('83, '84, '86), who maintained, furthermore, that close inbreeding interferes with embryonic nutrition, by lessening the vitality of the mother, and so produces a great excess of male young.

Helen Dean King has recently reported some results of the effect of inbreeding upon the production of male or female offspring among albino rats. Her conclusions in part are as follows:[1]

In each series the difference between the sex ratio for the group of inbred litters and that for the group of half-inbred litters is a significant one. Apparently, therefore, the chemotactic reaction between the ovum and the spermatozoön is not quite the same where these sexual elements come from unrelated individuals as when they are produced by individuals that are closely inbred.

Morgan ('14) has suggested that the infertility of the eggs of Ciona to spermatozoa from the same individual may be due to the similarity in the hereditary complex of the germ cells, which in some way decreases the chances of their uniting. The selective fertilization experiments made by Marshall ('10) with different varieties of dogs and also my own experiments with different varieties of rats show that the ova of these animals have a strong tendency to unite with spermatozoa from individuals belonging to unrelated stock rather than with spermatozoa from individuals of the same blood.

The results of this series of experiments, as a whole, seem to indicate that in the rat, as in the pigeon (Riddle, '14, '16, '17), in Drosophila (Moenkhaus, '11) and in the guinea-pig (Papanicolau, '15), the female has more influence in determining the sex ratio than has the male. Yet it is not in the differentiation of the ova, nor in the development of the spermatozoa, that the key to the riddle of sex-determination will be found. A knowledge of the interaction of the germ cells, and of the conditions that influence it, must be gained before the final solution of this problem can be attained.

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[1] Helen Dean King. Studies on Inbreeding—III. The effects of inbreeding, with selection on the sex ratio of the albino rat. *Journal of Experimental Zoölogy*, vol. 27, No. 1.

THE FOUNDERS OF THE ART OF BREEDING—IV

Pre-Mendelian Breeders of the Nineteenth Century—*Concluded*

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DURING the time of the prosecution of the work of Knight and Herbert appeared the results in hybridization obtained by Sageret in France. Sageret's experiments in crossing were largely confined to the Cucurbitaceae, and his results were published in a memoir entitled, "*Considérations sur la production des hybrides, des variantes et des variétés en général, et sur celles de la famille des Cucurbitacées en particulier*," which appeared in 1826 in the *Annales des Sciences Naturelles*, Vol. 18.

SAGERET ON SEGREGATION

Sageret made some discoveries that clearly anticipate our modern knowledge of segregation, and he was able to furnish what was, for the time, a fairly satisfactory scientific explanation for the reappearance of ancestral characters. The experiment upon which his conclusions were primarily based was a cross in which a muskmelon was the female and a cantaloupe the male parent. Each plant was regarded as a relatively pure or type representative of its kind. In stating the results of the cross, Sageret for the first time, so far as the writer knows, in the history of plant hybridization, aligned the characters of the parents in opposing or contrasting pairs after Mendel's fashion forty years later. Following is the list of contrasting parental characters as Sageret gives them:

Muskmelon (female)

1. Flesh white.
2. Seeds white.
3. Skin smooth.
4. Ribs slightly evident.
5. Flavor sugary, and very acid at the same time.

Cantaloupe (male)

1. Flesh yellow.
2. Seeds yellow.
3. Skin netted.
4. Ribs strongly pronounced.
5. Flavor sweet.

Sageret remarks: "The assumed product of the crosses made ought to have been intermediate: (1) Flesh very pale yellow, (2) seeds very pale yellow, (3) netting light, (4) sides slightly marked, (5) flavor at once sweet and sprightly, but the contrary was the case."

As a matter of fact, in the two hybrid fruits reported upon, *the characters were not blended or intermediate at all, but were clearly and distinctly those of the one or the other parent.*

First hybrid	*Second hybrid*
1. Flesh yellow.	1. Flesh yellowish.
2. Seeds white.	2. Seeds white.
3. Skin netted.	3. Skin smooth.
4. Ribs rather pronounced.	4. Ribs wanting.
5. Flavor acid.	5. Flavor sweet.

In the further support of his conclusions regarding the descent of characters in unitary fashion, he remarks upon the inheritance of human hair and eye color in the mating of a brunette with a blonde type. He comments upon the fact that these two hybrids are types of which he had "several times obtained the analogues or their equivalents." While there is fusion here and there, he says, "one sees here a much more marked distribution of their different characters, without any mixture between them." He even uses, for the first time in the literature of plant hybridization, the word "dominate" with reference to characters in crossing, in the following words. Speaking of the inheritance of flavor in various melon crosses, he says:

"The acid flavor of the muskmelon is encountered in the forms of the cantaloupe and the snake-melon; in others, the form of the cantaloupe dominated."

Summing up the results of his experiments in a general conclusion, he says, with regard to the natural expectation that in a hybrid there will be a complete or partial fusion of the parental characters, that "this fusion of characters may take place in certain cases; but it has appeared to me that, in general, things did not take place in this way;" and again: "It has appeared to me that, in general, the resemblance of the hybrid to its two ascendants consisted, not in an intimate fusion of the diverse characters peculiar to each one of them in particular, but rather in a distribution, equal or unequal, of the *same characters*." (Italics inserted.)

Here we meet again, for the first time in the literature of hybridization, the phrase "distribution of characters" now so familiar. "These facts," Sageret remarks, "have been confirmed by a multitude of my experiments." It is evident from the following statement that Sageret appraised his discovery of the dominance of characters in crossing at its proper value: "The ideas which I present," he says, "have appeared remarkable to me; they seem to me to be of a very great importance."

In addition to his melon crosses, Sageret secured a hybrid between a black radish and a cabbage, of which he says: "The fruits, instead of being intermediate, were like either cabbage or radish on the same inflorescence. Each silique bore a single seed, analogous to its pod," to which he makes reference in a further comment upon "*the distribution among hybrids, of the characters of their ascendants without fusion of these characters*," a point of view with regard to the results of hybridization that needs little to make it modern. It is, however, a matter of further interest that Sageret was able to derive the natural scientific conclusion from the facts of unit-character inheritance as he found them, with respect to the reappearance of old or the appearance of new "species." The hybrids "often reproduced for me," he says, "varieties which had long ago disappeared."

He finally concludes: "To what, then, does this faculty belong, which nature has, of reproducing upon the descendants such or such a character which had belonged to their ancestors? We do not know. We are able, however, to suspect that it depends upon a type, upon a primitive mould, which contains the germ which sleeps and awakens, which develops or not according to circumstances, and possibly that which we call a new species, in which develop organs, ancient but forgotten, of which the germ existed, but which the development had not yet favored."

DARWIN ON HYBRIDS

On November 24, 1859, appeared the first edition of Darwin's epoch-making book, "The Origin of Species," in which he briefly reviewed (Chap. 9) the results and conclusions regarding hybrids and hybridization up to his time. In reading Darwin's chapter one is strangely struck by the persistence of the species-variety question.

Is this a "species," or is it merely a "variety?"—a question which crossing was expected to answer. If two organisms would not cross, or if their offspring were sterile, they were thereby proved to be distinct "species." If they freely intercrossed, or if their offspring were fertile, then, *ipso facto*, they were "varieties" of the same species. Darwin's thesis—that "species," so called, grew out of "varieties" so called, by natural selection, caused him to review the evidence which the work of the hybridists, especially Koelreuter, Gartner and Herbert, afforded. Regarding the matter of the relation of hybrids to species-affinity, Darwin writes with his usual conservative wisdom:

"No one has been able to point out what kind or what amount of difference, in any recognizable character, is sufficient to prevent two species crossing. It can be shown that plants most widely different in habit and general appearance, and having strongly marked differences in every part of the flower, even in the pollen, in the fruit and in the

cotyledons, can be crossed . . . (p. 14). The facility of making a first cross, between any two species is not always governed by their systematic affinity or degree of resemblance to each other. This latter statement is clearly proved by the difference in the result of reciprocal crosses between the same two species, for according as the one species or the other is used as the father or the mother, there is generally some difference, and occasionally the widest possible difference, in the facility of effecting an union. The hybrids, moreover, produced from reciprocal crosses often differ in fertility" (p. 16).

Again he says: "There is often the widest possible difference in the facility of making reciprocal crosses. Such cases are highly important, for they prove that the capacity in any two species to cross is often completely independent of their systematic affinity, that is, of any difference in their structure or constitution, excepting in their reproductive systems (p. 14). It can thus be shown that neither sterility nor fertility affords any certain distinction between species and varieties. The evidence from this source graduates away, and is doubtful in the same degree as is the evidence derived from other constitutional and structural differences" (p. 4).

Darwin finally summarizes the evidence as follows: "First crosses between forms, sufficiently distinct to be marked as species, and their hybrids, are very generally, but not universally sterile. The sterility is of all degrees and is often so slight that the most careful experimentalists have arrived at diametrically opposite conclusions in ranking forms by this test" (p. 44).

In 1861 the Paris Academy of the Sciences proposed the following problem to receive the grand prize in the physical sciences: "To study plant hybrids from the point of view of their fecundity, and of the perpetuity or non-perpetuity of their characters. "The production of hybrids among plants of different species of the same genus is a fact determined a long time since, but many precise researches still remain to be made in order to solve the following questions, which have an interest equally from the point of view of general physiology, and of the determination of the limits of species, of the extent of their variations.

"1. In what cases of hybrids are they self-fertile? Does this fecundity of hybrids stand in relation to the external resemblances of the species from which they come, or does it testify to a special affinity from the point of view of fertilization, as has been remarked regarding the ease of production of the hybrids themselves?

"2. Do self-sterile hybrids always owe their stability to the imperfection of the pollen? Are the pistil and the ovules always susceptible of being fecundated by a foreign pollen, properly selected? Is an appreciable imperfect condition sometimes observed in the pistil and the ovules?

"3. Do hybrids which reproduce themselves by their own fecundation sometimes preserve invariable characters for several generations, and are they able to become the type of constant races, or do they always return, on the contrary, to the forms of their ancestors after several generations, as recent observations seem to indicate?

THE IDEAS OF GODRON

The two chief competitors under the Academy's offer were Charles Naudin of the Museum of Natural History at Paris, and D. A. Godron of the University at Nancy, the prize being awarded to the former. The papers of both appeared in Vol. 19 of the *Annales des Sciences Naturelles* (Botanique), 4 me. Serie (1863).

The title of Godron's thesis was "*Des hybrides végétaux, considérées au point de vue de leur fécondité et de la perpetuité ou non-perpetuité de leurs charactéres.*" His paper is chiefly devoted to the solution of the question as to whether "hybrids reproducing by self-fertilization sometimes keep their characters invariable during several generations, and whether they are able to become the types of constant races, or whether, on the contrary, they always return to the forms of one of their

ancestors at the end of several generations, as recent observations seem to indicate. In answer to this query, he says: "We have determined, upon hybrids of *Linaria*, that the hybrid forms may become very fertile, and that a certain number of individuals, from the second generation, return respectively to the two primitive types, when they grow in company with their parents, and this return movement manifests itself much more in the following generations."

Godron remarks that the same fact has been observed by Lecoq in the fertile hybrids of stocks, by Naudin in the fertile hybrids of tobacco, and by several observers in primula and in petunia. From these experiments, then, he concludes the proof of the final return of fertile hybrids to their parental forms to be established. Godron was a victim of the rigid idea of species, which held, that because so many hybrids between different "species," so called, were sterile, that therefore any hybrid which proved fertile must necessarily, *ipso facto*, prove the parents not to be of different species but to be merely varieties of the same species.

To the vain purpose of settling this verbal controversy, as to whether such and such plants were to be regarded as separate "species," or merely as varieties of the same species, many of the most ardent endeavors of hybridists, both before and since Mendel's time, have been conscientiously and duly devoted. A sample of this method of reasoning in a circle so vigorously combated by Herbert, and characterized by Sageret as "fighting the air," is exemplified in a sentence of Godron which typifies the then general view. He says: "*To admit that two distinct species have produced hybrids which, from the very first have become very fertile, would constitute a very grave exception to the law* which has its sanction in the numerous experiments which, for a century past, have been made by Koelreuter, Wiegmann, C. F. Gartner, etc., and by M. Naudin himself, *that simple hybrids are sterile or but little fertile.*" (Italics inserted.)

Considering the fact, however, that hybrids between confessedly distinct species are so frequently sterile, it is not surprising that, in view of the then greater interest in the species question itself, that hybridizers should have turned systematic botanists and have made the sterility of the hybrid offspring a criterion of species distinction. Besides his competing memoir before the Paris Academy, Godron was the author of several other contributions to the literature of plant hybridization, including that of the then celebrated question as to the possible origin of cultivated wheat from the wild plant *Aegilops ovata*.

NAUDIN'S CONCLUSIONS

The general conclusions of importance for his time, at which Naudin arrived, are as follows—in the language of the award committee of the Academy—and which are quoted in their own words (6c) to show the point of view of science at that time: "The first, and the most important of all, is that the singular beings which result from the cross-fertilization of two different types, far from being condemned to absolute sterility, are frequently endowed with the faculty of producing seeds capable of germination" (p. 129).

"The second consequence of major interest which proceeds from the numerous experiments in the same memoir is *that fertile hybrids have a manifest tendency to return to the forms that produced them, and that without other action than that of their own proper pollen, under such conditions that the pollen of the parents is not able to exercise the influence to determine this return*" (p. 129).

UNIT CHARACTERS

An essential feature in Naudin's paper, of high importance from our present standpoint, is the independent behavior of characters in a cross, and referred to by the Academy committee as follows: "Not content with responding by numerous experiments to the questions propounded by the Academy, the author . . . has sought to throw light upon several points, some obscure, others not yet studied, in the

history of hybrids. He has confirmed that which Sageret already knew, that in a hybrid the characters of the two parents are often shown, not blended but approximated, in such fashion that the fruit of a *Datura* hybrid, born of two species, the one with a smooth, the other with a spiny capsule, *presents smooth places in the midst of a surface generally spiny*. This disjunction, as it is called, is explained according to him by the presence in the hybrid of two specific essences, which tend to be separated more or less rapidly the one from the other. He even sees in this disjunction the true cause of the return of fertile hybrids to the types from which they came" (p. 131). (Italics inserted.)

It is further of great interest to note that the seeds gathered from the smooth side of the capsule reproduced only the smooth-capsule form, *Datura laevis*, while those taken from the spiny side gave rise only to the spiny form, *Datura stramonium*. In Verlot's paper, yet to be discussed, further instances of this vegetative segregation, as it may be called, will be found.

Naudin stated, more clearly and definitely than others had hitherto done, the fact of the general uniformity of the hybrid offspring of the first generation (the F_1 generation), as we should say, and the diversity of form, with partial reversion to, or, as we would now put it, the reappearance of, the parental types in the second hybrid (or F_2 generation). His language is as follows: "Finally, one may say that the hybrids of the same cross resemble one another in the first generation as much, or almost as much, as the individuals which come from a single legitimate species."

In contradiction to the results derived by Sageret from his particular set of experiments, Naudin asserts the generally intermediate nature of the first generation hybrids: "All the hybridologists are in accord in recognizing that the hybrids (and it is always the question of the hybrids of the first generation) are mixed forms, intermediate between those of the two parent species. This is, in fact, what takes place in the immense majority of cases; but it does not follow therefrom that these intermediate forms are always at an equal distance from that of the two species." He goes on to remark upon the vagueness with which this relative approximation is determined, resting as it does, largely upon a basis of opinion. He also calls attention to the fact that sometimes the hybrids resemble one of the two parents in certain parts and the other in other parts. Regarding segregation in the second hybrid generation he says: "Very often, to the so perfect uniformity of the first generation, there succeeds an extreme medley of forms, some approaching the specific type of the father, the others that of the mother . . . It is, as a matter of fact, in the second generation that this dissolution of the hybrid forms commences in the great majority of cases. . . . Among several of these hybrids of the second generation, there is a complete return to one or the other of the two parental species, or both, and diverse degrees of approach to these species."

NAUDIN'S EXPLANATION

Naudin now comes to what he regards as the philosophical explanation of these facts: "All these facts are naturally explained by the disjunction of the two specific essences, in the pollen and in the ovules of the hybrid. A hybrid plant is an individual in which are found united two different essences, having their respective modes of development and final direction, which mutually counter one another, and which are incessantly in a struggle to disengage themselves from one another." "The hybrid," he says, "in this hypothesis would be a living mosaic, in which the eye would not discern the discordant elements as long as they remained intermingled; but, if in consequence of their affinities, the elements of the same species mutually approximating one another, agglomerate in rather considerable masses, there may result therefrom parts discernible to the eye, sometimes entire organs, etc."

Naudin concludes that the pollen and the ovules, and the pollen especially,

"are the parts of the plant where the specific disjunction takes place with the most energy." He goes on to suppose, and here, perhaps he comes closest to a statement of Mendel's view, "that in the hybrids of the first generation the disjunction takes place at the same time in the anther and in the contents of the ovary; that some of the grains of pollen belong totally to the species of the father, and others to the species of the mother; that in others the disjunction has not occurred or has just commenced; let us grant again that the ovules are, in the same degree, segregated toward the side of the father and toward the side of the mother. . . . If the tube from a grain of pollen, approximated to the species of the male parent, encounters an ovule segregated in the same direction, there will be produced a plant entirely reverted to the paternal species. The same combination being accomplished between a grain of pollen and an ovule, both segregated in the direction of the female parent of the hybrid, the product will return in the same way to the species of the latter; if, on the contrary, the combination is effected between an ovule and a grain of pollen segregated in a direction contrary the one to the other, there will result a true cross-fertilization like that which has given birth to the hybrid itself, and there will result therefrom a form intermediate between the two specific types."

In 1864 Naudin communicated a second report to the Academy, in which he confirmed his previous results as to uniformity in the first generation crosses, the identity of reciprocal crosses, and the "disorderly variation," as he calls it, of the hybrids of the second and succeeding generation. In neither of the two papers is there any numerical classification of the hybrid types.

Naudin's memoir is often referred to as amounting virtually to a statement of Mendel's law of the disjunction of hybrids. In Naudin's case, however, the statement was of a speculative nature and consisted in the proposition of a scientific hypothesis; in Mendel's case, his "law" was a scientific conclusion derived as the result of experiment. Naudin propounded, in 1863, a well-reasoned theory of probable truth; Mendel, in 1868, formulated a statement of ascertained fact.

THE WORK OF VERLOT

In 1865 B. Verlot, of the Jardin des Plantes at Paris, published a brief memoir which in 1862 had received a prize from the Imperial and Central Horticultural Society, the thesis of which was as follows: "To demonstrate the circumstances which determine the production and fixation of varieties in ornamental plants." The memoir is of interest as thoroughly and typically embodying the general point of view of the day concerning hybridization and the origin of new varieties, while affording at the same time much matter of interest from the standpoint of practical horticulture. Verlot presented the view that, while the causes of variation are unknown, they arise under definable circumstances, chief among which he enumerates prolonged cultivation, removal from one set of climatic and soil conditions to another, and hybridization.

The thought of the time did not clearly distinguish a difference between the nature of the changes brought about by the external environment and those arising from sexual fertilization. Both were generally assumed to be equally heritable. Cultivation long continued was considered to have been especially potent in bringing about variation. In Verlot's words: "It is especially with plants cultivated for a great number of years, with those the introduction of which is so ancient that it is lost in the night of time, that one finds profound and multiplied modifications" (p. 4).

He further voices the then prevailing view regarding the relation between culture and variation: "If we compare," he says, "a species in its spontaneous condition with the same species cultivated, transported, that is to say, most often into conditions of climate, soil, etc., completely different from those where it lived before, we shall be struck by seeing that in our gardens this latter will show deviations of type more numerous than in the wild state. We

shall derive from this fact the consequence that the faculty of varying, which is proper to the plant, augments with culture. If we observe, then, that the plants cultivated in our gardens which have varied the most—as, for example, the dahlias, the roses, the camellias, the rhododendrons, the potato, etc.—are not borrowed for the most part from our flora, nor from one of the neighboring floras, but on the contrary come from distant countries, where they grow under conditions often absolutely different from those in which we cultivate them, we shall conclude that the more a species is depatriated the more it will easily vary" (p. 30); and again: "The more plants are cultivated, the greater their variations are, and, by the same token, the easier they are to fix. We will possibly be contradicted, but we do not hesitate to consider, once more, long-practiced culture as one of the most favorable antecedents to the rapid fixation of variations" (p. 38).

We now know at least that the increased variation manifested by wild plants, when brought into cultivation, is probably due to the removal of the restrictive influences of competition rather than to any actual increase in the range of heritable variability itself.

Verlot cites, as examples of the changes supposedly wrought by culture, the changes brought about in the roots of such plants as beet and parsnip, in the production of dwarf plants, in various modifications in general habit, such as fastigiate, pyramidal and weeping variations in trees; in the appearance of variations with laciniate or otherwise modified leaves; in varieties with leaves colored white, yellow, red or brown; in the arrangement of the leaves, as in the sudden appearance on an ordinary alternate-leaved plant of *Rosa alba*, of a shoot with opposite leaves, propagated as *Rosa cannabifolia*. From the evidence he concludes that cultivation sets up within the plant a condition of instability, which gives rise not only to seed variation but to variation within the plant itself—what we would call bud-variation or somatic segregation—as in the case just cited, the case of a chrysanthemum reported which bore at the same time yellow and rose-colored flowers, and of a citrus fruit half-and-half orange and lemon. Another case cited by Verlot is that of a variegated *Camellia imperialis* which, for twelve years, had constantly given brilliant white flowers set off with rose-colored striations and variegations, and upon which a small branch appeared one year bearing three flowers, in a group of a uniform rose color, the same in tint as that of the striations and variegations of the other flowers.

"It is evident, in these cases," says Verlot, "that the colorations disjoin and that this variation returns by disjunction to its colored types, as we have indicated for certain plants of hybrid origin. (p. 67). As we see," he says, "by the sole fact that a plant is cultivated, it is forced to vary. The instability of a cultivated plant is even evident in certain cases in such a way that it does not only manifest itself in the direct descendants of the plant, but also in the plant itself. Thus, while the generality of the branches of a plant bear leaves, flowers and fruits of definite forms or colors, a branch is sometimes produced in which the leaves, flowers, and fruits present completely different characters. We recognize that culture has been and is still the essential cause of the variation of plants, and that thereby man has, so to speak, compelled them to re-clothe themselves with new forms appropriate to his needs or to his caprices" (p. 5).

The above statement excellently presents the older point of view regarding variation. Such cases as the rose, chrysanthemum and orange, and the famous hybrid *Cytisus Adami* (*purpureus* × *laburnum*), Verlot accounts for under the guise of Naudin's conception of "disjunction": "It is by disjunction that, in these last cases, the specific forms thus reappear in hybrid plants, and it is with woody plants, it will be noticed, that this fact presents itself; individuals, that is to say, which, persisting for long years, must achieve all the phases of existence of a hybrid plant,

an existence of which this disjunction would be the last term" (p. 14). He then refers to Naudin's case of disjunction in *Datura*, which is elsewhere discussed. Verlot's expression of views on the matter of methods of selection is so thoroughly typical of the thought of his time, *i. e.*, that variation is in consequence of the "breaking up" of the "type," and that selection, *ipso facto*, intensifies the variation in the direction selected for, that it is a matter of interest to present here the view expressed. "If a variation is produced in a direction other than that toward which one tends, it ought not to be abandoned for that; one will have more chance of obtaining new variations in sowing a deviation from the type, even in a diametrically opposite direction, than in sowing anew the type itself. In the deviation there is already a tendency toward perturbation, and toward the beginning of the destruction of atavism" (p. 31).

Another interesting example of the older empirical point of view regarding plant improvement is Vilmorin's opinion, quoted by Verlot, and which is here reproduced to show how thoroughly the primary idea of "breaking up the type" in order to bring about "variation" entered into the thought and operations of pre-Mendelian breeders.

"To obtain from a plant not yet modified, varieties of a kind determined in advance, I will first set myself to making it vary in some direction or other, choosing for the reproducing factor, not that one of the accidental varieties which would most nearly approach the form which I have proposed to myself to obtain but simply that which would most differ from the type. In the second generation, the same care would make me choose a deviation, the greatest possible at first, the one most different, in a word, from that which I would have chosen in the first place. Following this direction for several generations, there necessarily ought to result, in the products obtained, an extreme tendency to vary; there then results again, and that is the principal point according to me, that the force of atavism, exerting itself counter to very divergent influences, will have lost a great part of its power, or, if one ventures to make use of this comparison, it will exert it always in a broken line" (p. 28).

Man's relation to the fixation of characters in new races of plants is stated by Verlot in the usual manner prevalent in the days before Mendelian analyses: "In brief, gardeners have remarked with reason that a plant newly introduced is very susceptible to vary. This fact, it is conceived, has nothing surprising about it. It confirms that which we have previously said, that a variety, whatever it might be, had need, in order to become fixed, of being cultivated for a greater or less length of time, until one had finally come to maintain with it the tendency not to depart from being that which he had made it" (p. 70).

In other words, the idea then prevalent, and more or less incoherently expressed, was that, in some unknown manner, man, by continued selection, succeeds in impressing upon a "variety" the stamp of a certain type, and, through repeated and continuous selection in the same direction, finally "fixes" it, so that the variety becomes, as it were, stabilized. Analyzed in a modern way, it simply means that, by continuous selection of some certain type, those individuals are usually isolated which are homozygous for the character-units thus represented, and which become "fixed," because no heterozygous factors are left to split apart.

We have here, in other words, an unscientific sensing through practical experience of the fact which the breeder of today would define as the selection of a heterozygote having dominant characters differing from those of the species. Being of hybrid nature, such a plant would break up and hence yield new types, whereas the plants resembling the type are more apt to be homozygous and less liable to vary in their progeny. He emphasizes the view just set forth still more emphatically in the following words: "If two variations are produced, of which the one differs little

from the type, but is placed upon the line which leads in the desired direction, and the other is placed in an opposite direction, but departing considerably from the type, we shall not neglect nevertheless to follow this latter, because with it the breaking-up of atavism is more advanced" (p. 31).

The necessity of fixing upon some single individual plant as the basis of selection is referred to by Verlot in the following terms: "We ought, then, to recognize that it is necessary to take account for the choice of the seed-bearers, not only of the external characters but even of the idiosyncrasy of each one of them. Now since this does not manifest itself except by its effects, we shall, if a variation seems to present some difficulties in becoming fixed, have to examine separately the products of each of the seed parents and make our choice bear upon those which present, in the least pronounced degree, atavism, or the tendency to return to the primitive type" (p. 32).

Verlot's experience with and observations upon hybrid plants, as coming from an experienced horticulturist, are interesting, and, to the practical plant breeder, valuable. Regarding the now well-understood fact of the gradual disappearance of the hybrid forms through segregation, he says: "Their fertility is of short duration, through the more or less rapid return of their products to the types which have given them birth" (p. 25).

Regarding the general aspects of plant hybrids, he adds: "All their characters, of whatever nature they may be, with the exception of a more considerable development of the organs of vegetation, are, in general, intermediate between those of the parents, but always limited by them" (p. 25). Regarding the matter of the bounds or limits of the hybrid characters he remarks elsewhere: "Let us call attention to a circumstance always constant in the hybrids, which we have to consider, that is, the absence in the products of colors other than those, or a combination of those of the parents. We shall insist upon this characteristic, because we shall have occasion to recur to it; it will serve us to establish the fact that up to now the facts prove that, by hybrid fecundations, one will obtain, in whatever part of the plant they present themselves, the variations of color only, limited to those of the parents" (p. 18).

Since Verlot's view regarding the nature of a hybrid was the conventional one that it consists of a cross between what are commonly called distinct species, he was led to notice the very common fact of comparative sterility in these cases. Noting the well-known characteristic of augmented vegetative growth in hybrids, he is led to ascribe the seed sterility to the latter, a conclusion easily if naïvely arrived at from the well-known inverse relation between undue vegetative luxuriance and seed reproduction. As an instance of intermediacy, Verlot alludes to the matter of height: "In crossing an almost dwarf species with the pollen of a taller species . . . the seeds of this cross will undubitably produce individuals taller than was their mother" (p. 44).

Regarding intermediateness in size in flowers, he says: "In crossing a species *parviflora* by its variety *grandiflora* we shall be able . . . to obtain individuals with flowers larger than those of their mother . . . by crossing one is able then to create a race or a variety in which the size of the flowers will be augmented" (p. 47). With regard to the same matter in respect to earliness and lateness, he says: "Supposing one crosses a very early plant with its very late variety, or *vice versa*, one will only be able to obtain varieties intermediate between the parents in earliness or lateness" (p. 50).

Regarding fragrance he mentions the case of a cross between *Rhododendron ciliatum* (odorless) and *R. Edgeworthii* (very fragrant), the hybrid being less intensely fragrant than the pollen parent (p. 31). In the matter of color intermediateness he makes the statement: "Once obtained, white coloration is able to serve, either by crossing or by hybridization, in the production of new variations, ordinarily intermediate

between it and the color from which it has proceeded" (p. 59); in other words, as we should say today, dilution through the presence of but a single dose of the color factor.

The most interesting portion of Verlot's memoir is his discussion of the practical results achieved with ornamental plants in the field of hybridization. Regarding dwarfing he cites McNab (p. 42), to the effect that the best dwarf varieties of rhododendron are obtained by the use of pollen taken from the small stamens, "the products of which," he says, "I am able to certify, are very different from those obtained by the use of the pollen of the large stamens." Regarding breeding for winter hardiness, he mentions the case of the cross of *Amaryllis brasiliensis*, a delicate species impossible to winter out of doors, by *Amaryllis vittata*, a much hardier plant, whereby hybrids were produced which, with light covering, would support the climate of Paris. Likewise *Rhododendron arboreum*, which cannot resist more than 2 to 3 degrees of cold, gave, when crossed by *R. catawbiense* (a much hardier form, though with inferior inflorescence), hybrids which inherited the hardiness of the female plant.

Verlot did not recognize the phenomenon of dominance in the first generation of the hybrids, but he mentions the case of a white *Gloxinia* crossed by pollen from a blue-flowered variety, in which out of one thousand seedlings "all bore nothing but perfectly blue flowers, not a single one of them being white, nor a single one variegated" (p. 65). Likewise the crossing of the same plant by a red-flowered variety gave the same result, "all the plants coming from sowings of seeds thus produced had entirely blue flowers" (p. 65).

Regarding the inheritance of variegations it may be of simple interest to note that the following species are mentioned in which the variegated form breeds true from the seed. *Alyssum maritimum*, *Barbarea vulgaris*, *Celtis australis*, *Cheiranthus cheiri*. With these are to be included the variegated ferns *Pteris argyraea* and *P. aspericaulis* var. tricolor.

He remarks upon an interesting fact that the variegations do not appear upon the first leaves of a variegated variety. Regarding the heredity of double flowers, he reports no crosses, but simply remarks upon cases of double-flowered peach and apple which came true from the seed (p. 83).

Another interesting piece of information given is that, in the case of double camellias, the full round seeds produced plants with very double flowers, while the more elongated seeds produced plants with single or but slightly doubled flowers (p. 87). Among color variations in plants, few are more interesting than the red or copper-leaved forms of ordinary green-leaved types. It may be of interest here to note Verlot's citations of such color varieties as are homozygous for the color, and which hence come true to type, among the recorded instances being the purple beech and the purple-leaved barberry.

One of the most interesting matters recorded in Verlot's paper, is his citation of Bridgmann's investigations on the inheritance of leaf variation in ferns. Many species of ferns show various degrees of leaf laciniation, the latter becoming often many times compounded. In the case of several species of *Scolopendrium* with crisped or laciniate leaves, spores were separately sown from the modified and from the unmodified leaves, with the result that the resulting plants, although derived from the same original plant as a parent, inherited the characteristics of the particular leaves or parts of leaves from which the spores were taken.

"The spores from the deformed part of the frond were collected and sown separately. . . . The result was that all the plants which came from them reproduced the crisped form of the individual mother plant, and some of them even in a higher degree" (p. 98).

"The spores from the normal part of the frond, which had furnished the first sowing, were collected with the same care, and sown under identical conditions. There originated in the same way thousands of young plants, but it was scarcely possible in the

quantity of them for one to find a dozen which showed, even to a feeble degree, the irregularities of form so characteristic of the first lot. The two sowings were so different from one another that, if one had not known their derivation, one would never have been able to believe that they were such near relatives. The great majority here were perfectly normal; as to the small number of those which showed traces of the maternal monstrosity, this monstrosity was limited to fronds bi-lobed or tri-lobed at the top, with the edges more or less sinuous or a little slashed; yet this alteration most often did not reach more than one or two fronds on the same individual" (p. 98).

VERLOT'S SUMMARY

Verlot summarizes his views upon hybrids in the following words, which are worth reproducing because they fairly well represent the general knowledge of the time, as follows: (1) "Hybrid fecundation is not able to produce anything but variations which will be able, it is true, to multiply themselves mechanically, but which will not be fixable, and which consequently cannot be brought to constitute races or varieties, the products which arise from them being sterile, or if they are fertile, having only a fertility limited to a few generations, or disappearing after a certain time by the disjunction of the types. (2) One of the characters of the hybrids is also a great development of the vegetative organs, coincident with less abundant flowering. They are in general intermediate between the species types, but often approach more the father. (3) The hybrid, self-fertilized, returns more or less rapidly to the parents. (4) The hybrid, fertilized by a parent, returns also promptly to the parent. (5) Crossing—that is to say, reciprocal fertilization of varieties or races of the same species—will serve for obtaining new variations, intermediate between the parents, very fertile, and which can be fixed more or less rapidly, and constitute new varieties or races."

Reviewing this list of statements in the light of present knowledge, we can see that they constitute a more or less correct, non-scientific formulation of the truth. For example, the more or less rapid return of hybrids—that is to say, heterozygotes—to the parental forms, is well established today as a fact of segregation according to Mendelian ratios, which, if there is a single pair of allelomorphs in question, goes on, on a 1.2.1 basis in each successive generation. The more or less rapid return to its parents of the hybrid fertilized by a parent is simply the splitting off of 50 per cent dominants or recessives as the case may be, and which are the parental types in the case of simple ratios.

WICHURA'S WORK

In 1865 there appeared Wichura's memoir on the hybridization of plants (10), based upon experiments in the crossing of willows, which had occupied him from 1852 to 1858 inclusive. A brief preliminary report had appeared in *Flora* in 1854, and also within the same year in the report of the *Schlesische Gesellschaft*.

Taken as a whole, Wichura's work dealt, not with the investigations of individual specific characters but with species taken entire and crossed as such. As was the general custom, he regarded a "species" as an integral whole that could be crossed in its entirety. With this conception he made what he called "binary," "ternary" and "quaternary" crosses, *i. e.*, crosses: (1) between two species; (2) between a species and a hybrid; and (3) crosses between two hybrids. Besides the smaller list of Wichura's successful crosses, he published a much longer one of his failures, which stand as evidence both of the considerable amount of crossing work that was done and of the scientific integrity of the experimenter. Of the ordinary, or, as he calls them, "binary" crosses, Wichura made, in all, thirty-five successful crosses and combinations of crosses (of which ten were "binary," *i. e.*, simply crosses in the ordinary sense) between twenty-one different species of willows.

Although, as has been stated,

Wichura, no more than most of the other hybridists of his day, paid attention to the crossing of characters as such, he remarks upon the evidence of individual characters being inherited as such: "It was of interest," he says (p. 27), "to observe how the unusual narrowness of the leaves in the experiment, utilizing *Salix purpurea* × *viminalis*, remained still recognizable in the following generation; a proof that, even in hybrid fertilization, individual characteristics of the parent plants can be inherited." (Italics inserted.)

Wichura noted in willows, as others had done in other plants, the fact of a higher degree of sterility on the part of hybrids obtained between species of more distant specific relationship. The greater amount of vegetative vigor of hybrids was remarked upon by Wichura in the following words (p. 40): "Not only in the reproductive organs, but also in their vegetative behavior, hybrids show many phenomena whereby they are more or less strikingly distinguished from true species. According to the corroborating observations of Koelreuter and Gärtner, a larger part of the hybrids obtained by them by hand crossing, were distinguished by luxuriance of growth. The plants grew to a greater height than the parents, spread out farther laterally by virtue of an increased capacity for sprouting, had a longer life-period, were able to withstand cold longer, and had more abundant, larger and earlier flowers than the parents. . . . Among the willow hybrids, similar phenomena occur, but the examples of luxuriant growth by no means constitute the rule."

Wichura further observed that: "Even the most sterile hybrids fall behind the parents in their productiveness. A certain deficiency in the parts set aside for reproduction must therefore also occur with them, and if we associate this in reverse relation with the excess of their vegetative development, it stands in complete harmony with the facts otherwise demonstrated. We shall therefore have to say, in order to express the relationship correctly, that in the case of very vigorous hybrids the weakness of the sexual parts brings out an increased development of the vegetative growth, whereas it is not the case with others which are too weak for such a reaction" (meaning crosses between too distant species) (p. 43).

Wichura concluded from his observations that hybrids were intermediate in respect to the differing parental characters. Cases of dominance do not seem to have come under his hand. "Among the numerous artificial and natural willow hybrids observed by me," he says, "I have throughout verified but one apparent exception to the principle of intermediateness. . . . Even the time of flowering of hybrids holds the mean between the time of flowering of the two parents" (p. 47). "The leaf-form of *Salix caprea* × *viminalis*, for example, holds so completely the mean between the linear-lanceolate leaves of *S. viminalis* and the round-ovate leaves of *S. caprea*, that they in fact appear to represent the mathematical mean between the curves for the outlines of paternal and the maternal leaves" (p. 47). With regard to what we should call absence of dominance, he has to say (p. 50): "As rich in species as the genus of the willows is, and as numerous combinations of hybrid fertilizations as it has to show, nevertheless I have never yet verified anything of a preponderant influence in any one of its species, but rather always found that their hybrids always hold the mean between the constant characters of the parents"; and again (p. 86): "In hybrid fertilization, if unlike factors unite, there arises an intermediate formation, etc."

The latter passage appears to be the first occasion where the term "factor" has been used in the literature of plant breeding, although here the factors referred to are the parents as a whole which participate in the cross, and not the character-forming elements of those parents. His general conclusion is (p. 46): "Constant characters, through which the parent species are distinguished from one another . . . go half over to the hybrid, so that it holds the middle position between them."

Two observations of Gärtner's were verified by Wichura—the identity of reciprocal crosses (pp. 51 and 186), and the fact of "variation" in hybrid progeny.

The question was as to the relative importance of the egg or the pollen in the result of fertilization. Wichura says (p. 57): "One sees the question is still far removed from having been brought to light, but from Gärtner's and my own observations it appears at least determined that the products of hybrid pollen in breeding are more various than those of the pollen of true species."

From the generally admitted identity of reciprocal crosses, Wichura draws the following conclusion (p. 86): "We have found that the products which come from reciprocal crossing, unlike the well-known experiments made in the animal kingdom, completely coincide with each other. *From this it follows, however, with mathematical necessity, that the pollen cell must have exactly the same share in the conformation of the fertilization product as the egg.*" (Italics inserted.)

So far as the writer knows, this is the first complete categorical statement in the literature of breeding of such a conclusion as to the behavior of the sex cells in amphimixis. One is completely impressed, in reading Wichura's work, with the scrupulous care, accuracy and precision with which his hybridization experiments were carried out. One or two passages in point are interesting. Referring to a case of Gärtner's, where exceptional types appeared in the midst of "a greater number of hybrid plants of completely similar hybrid types," he says (p. 53): "To judge concerning the here-mentioned exceptional types without myself having seen them is scarcely possible. From the relatively limited number of my experiments, which have not yielded the like, I cannot, to be sure, deny its possibility, but here likewise, as above in the case of reversions, the suspicion of a complete disturbance of the experiment, whether that the protection had not been complete, or the pollen utilized for fertilization not pure, or the seeds sown not free from foreign admixture. Whoever knows from his own experience how much care must be observed in order to keep an experiment clean becomes skeptical respecting all results of an experiment which vary from the usual rule, of the correctness of which one has not achieved conviction through his own observation."

Regarding these and other so-called anomalies as the result of crossing, he again says (p. 89): "That concerning all these points and many other disputable questions . . . we know so little, has indeed its basis in part in the method hitherto of artificial cross-fertilization, which suffers from the double deficiency that the care requisite to the correctness of the experiment, through the exclusion of foreign pollen, has not been taken in the first place, and second, that although many experiments have been instituted in very different families, nevertheless the individual hybrids have not been bred and observed in sufficient number. However, this is imperative throughout for attainment of general results. Only when one has at hand the same hybrid in hundreds of cases, partly from the same, partly from different parents, repeated through different years, only then will one be in position to separate the essential phenomena of hybridization from the more accidental ones."

Finally (p. 92), Wichura remarks, expressing the hope that a learned society or an individual with means might repeat his own experiments on a larger scale: "The most scrupulous exactness in such case would be indispensable. Failing this, and especially if the possibility of the access of foreign pollen is not completely excluded, then all experiments, the more extensively they are undertaken, only contribute so much the more to the confusion of the matter. This must be taken to heart."

Regarding the possibility of securing, in any given case, a cross, Wichura remarks (p. 84): ". . . only such species can be united in a hybrid which agree in relatively many characters, and correspondingly in many life conditions. Experience teaches the same thing in the familiar rule, that hybrid combinations

only occur between species of the same genus, or though different, yet nearly related genera." This statement represents a point of view resembling that held by Gärtner, as we have seen. He comes to a generalization of genetic value in the following statement (p. 85): "It is known that families die out, after a few generations, whose members carry in themselves the germ of a disease, and who marry only among themselves; and variety breeders know very well that all diverging characters of animal and plant species may be intensified, when, in successive fertilization, the precaution is taken that only similarly divergent individuals mate with one another."

This closes the account of the work in the field of hybridization from the time of Koelreuter to the time of Gregor Mendel, 1760 to 1866. Mendel's investigations, however, did not become generally known until 1900, so that very little change occurred in the methods pursued in the study of hybrid phenomena until after the date last mentioned. Comparatively few students of plant breeding, however, realize the historical value of the work of the earlier hybridists, in whose experiments lie the germs of our present knowledge.

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A COMMERCIALLY NEW BLUEBERRY

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M. A. SAPP lives 3½ miles from Crestview, in northwestern Florida, about half-way between Pensacola and River Junction. Some twenty years ago he conceived the idea of testing as a horticultural crop the wild plant known locally as the high-bush swamp-huckleberry. While Mr. Sapp has no technical horticultural training or experience in methods of plant breeding, he is one of those men who combines keen powers of observation with feeling for plants and ability to apply himself closely to a study of their requirements. Locally he is known as a particularly successful farmer.

Mr. Sapp visited a swamp not far from his place during the ripening season and tagged the blueberry bushes which appeared to him the most desirable from the standpoint of productiveness and size of fruit. In the succeeding autumn he transplanted one hundred of the bushes he had tagged to a well-drained field near his home, settting them out in rows spaced so that they would be 14 by 15 feet. This made about 200 bushes to the acre. He found that the suckers grew readily and that in their new environment they made better growth and fruited more profusely than they did in their native swamp. When he began to market the berries he encountered such an excellent demand, and was able to obtain such good prices, that he decided to extend his planting. He now has his original half acre, which is twenty years old; an acre which is twelve years old; 3 acres which are five years old; and 2 acres two years old, making a total of 8½ acres.

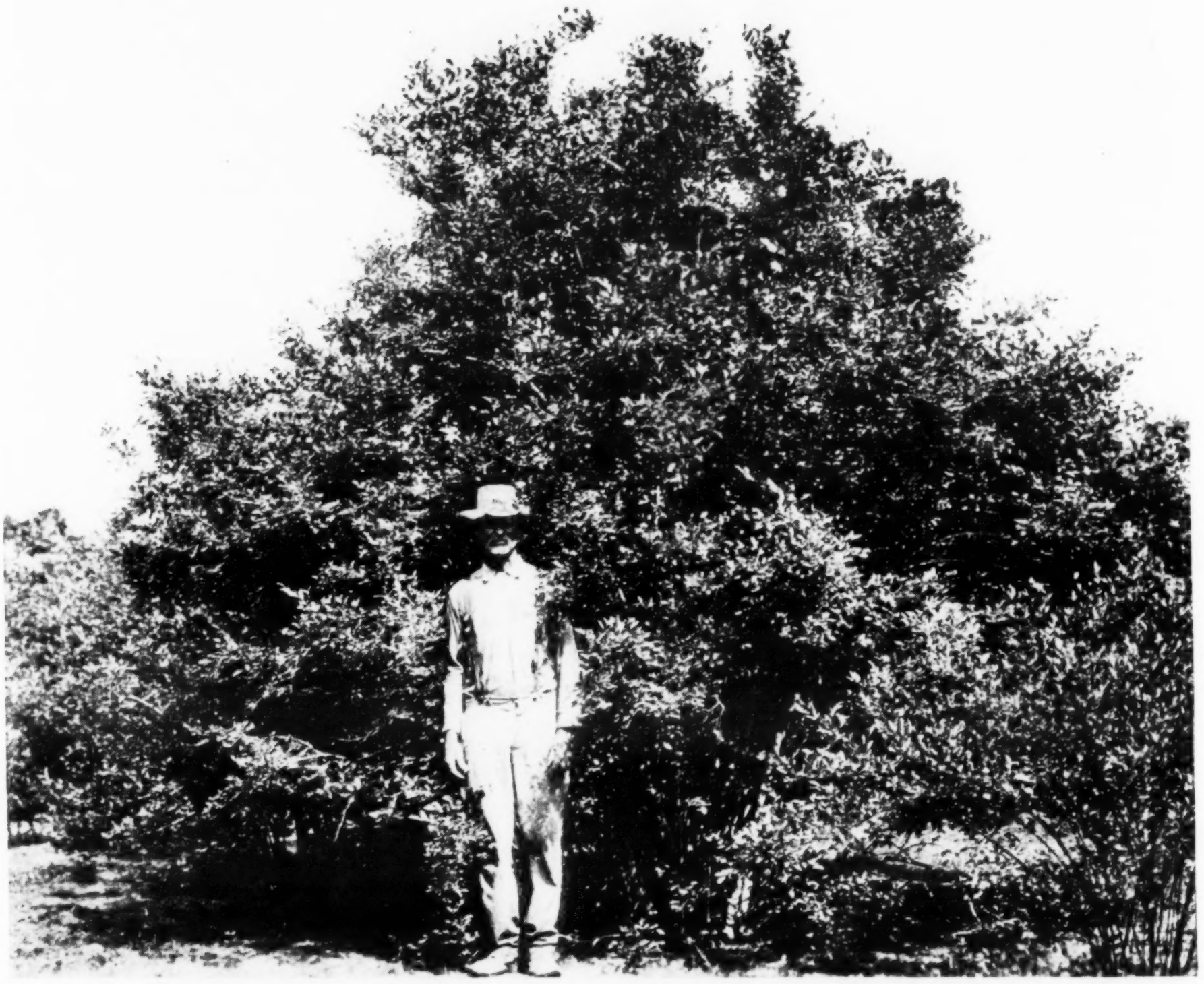
The plants commence bearing the second year after being set in the field, and at five years of age yield commercial crops of fruit. From the age of eight onwards they average half a bushel of fruit per plant annually, making the yield 3,200 quarts from an acre.

At the present time there is little apparent difference in productiveness between the twelve-year-old bushes and those twenty years old. There are, however, distinct types of plants occurring in the plantation. One type is stocky, compact, freely branching in habit, reaching about 8 feet in height and spread, with the fruit well distributed throughout the plant. The leaves of this type are ¾ inch by 1½ inch in size, lead-blue in color, with a distinct bloom upon the upper surface. The second type is erect in habit, many stemmed, 10 to 12 feet in height, with the fruit borne mainly on the ends of the branches. The leaves of this type are 1 inch by 2 inches in size, pale green in color. In addition to these two principal types there are several intermediate ones, varying in season of ripening and size of berry.

The botanical standing of this blueberry is somewhat in doubt. The description of *Vaccinium virgatum* Ait. appears to cover this species, yet there are certain differences which may, when the plant is studied more carefully, lead to its being classified differently.

Variation in productiveness is marked. In 1918, one bush yielded 20 quarts at a single picking, 3 bushels during the season. Others do not yield so abundantly. Mr. Sapp has found that the compact bushes are, in general, as productive as the tall ones, and the fruit is more easily gathered, hence he is confining his plantings at present to this type.

The season extends from May 20 to August 20, the bulk of the crop ripening in June and July. Pickings are made weekly throughout this period. The



A BLUEBERRY BUSH OF THE ERECT TYPE

Blueberry bushes vary in habit of growth. The one here shown is of the erect type; others are compact, spreading, and even drooping in form. This is considered by M. A. Sapp to be the most desirable type. A bush like this may yield 20 quarts of fruit in a single season. (Fig. 5.)

berries remain in good condition for several days and can be shipped without difficulty. They are sold on the farm at 10 to 12½ cents per quart. Up to the present Mr. Sapp has not attempted to ship to distant markets, as the local demand has exceeded the supply.

In diameter the berries vary from 10 to 14 mm. They contain seeds like those of other vacciniums, and in about the same quantity as the average blueberry of the north. The color is blue to black, the flavor sweet, pleasant, so agreeable that the fruit is satisfactory for eating out of hand as well as cooked. The unusually large size which is characteristic of Mr. Sapp's blueberries makes them of great interest.

The land upon which they are being grown is typical of thousands of acres of pine land found in the South Atlantic and Gulf states. The surface is sandy loam, the subsoil a sandy clay, with the water table 10 to 15 feet below the surface.

Stimulated by the success of the Sapp plantation, several other plantings have been made by people in the same section of Florida. D. T. Finlason, at Laurel Hill, some distance north of Crestview, is making a serious study of the matter, and has brought together on his place an extensive collection of varieties. In transplanting suckers from the swamps into the home garden, it has been found that they should be

ONE OF THE FIRST BLUEBERRY PLANTATIONS IN THE SOUTH

Part of the eight and a half acre plantation of M. A. Sapp in northern Florida. The bushes were transplanted from a neighboring swamp, and their culture has proved commercially profitable. Mr. Sapp has been able to improve the character of his fruit and increase productiveness by selecting the most desirable plants and propagating from them. (Fig. 6.)

A FRUITING BRANCH OF THE SOUTHERN BLUEBERRY

An acre of plants yields 3,200 quarts of these berries, and M. A. Sapp has found no difficulty in marketing them at 10 to 12½ cents a quart. The productiveness of the plants is indicated by this fruiting branch, here shown considerably reduced in size. (Fig. 7.)

set somewhat more deeply than they originally grew.

It is evident that the commercial possibilities of this fruit are excellent. All who are growing it state that the supply far exceeds the demand. Its culture is simple, it produces its fruit in the greatest abundance, and the prices obtained have been very satisfactory.

A Common Misconception Concerning Human Heredity

"Important as the heredity which has stamped its hall-mark upon an organism or an individual may be, there is another factor constantly at work in molding it, namely, its environment. The finished article, be it vegetable-marrow or man, is the resultant in a parallelogram of forces, the factors of which are heredity, or what it brings with it into the world, and environment, or the play of world forces upon it. So far as it concerns the individual, heredity stops at his birth, though the burden or the treasure it may have bound upon his shoulders is sometimes not revealed till after a long period of years. Its greatest influence is antenatal. The environment of the parents may affect the heredity of the child for good or ill, but the major part of the effect of environment is a post-natal and personal matter.

"A leek or a lily grown in the open air has green leaves, but if the plant is forced to live in a dark cellar where the sunlight cannot reach it, its leaves are white. It requires the energetic touch of the sun to enable it to elaborate the green chlorophyll, which gives it its verdure. Or it may be grown in the sunlight, in soil from which all traces of iron have been removed, and its leaves will remain pale. But if a little iron be added to the soil the leaves will quickly assume their natural hue. Here we are dealing with only one factor in the environment; but in nature, as a rule, the circumstances are more complex.

"The response to the conditions of environment are well shown by the behavior of certain Alpine plants. If taken from their natural habitat and cultivated in the lowlands, they undergo material alterations in character. They grow to a greater height and their leaves expand in length and breadth. So long as the plant remains in the lowlands it will exhibit in each successive generation these altered characters. But if one of the plants is transferred once more to its original habitat, high up upon the bleak mountains, it will once more assume the Alpine characteristics, which continue to persist so long as the plant or its descendants live under the same conditions. . . . In the nature of things it is perhaps to be expected that the influence of environment should make itself felt in plant life; but it plays a great part in producing modifications of animal life as well."

The above quotation from "The Adventure of Life," by Dr. Robert W. MacKenna, represents a mistaken assumption in regard to heredity that is very frequently encountered even in text-books on eugenics. It is an established generalization that the rôle played by environment in producing wide modifications compatible with life is far greater in plant life than in animal, and far greater on the simpler animal tissues than on the more complex.[1]

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[1] See Popenoe and Johnson, "Applied Eugenics," p. 3. New York, 1918.

THE DRAMATIC CAREERS OF TWO PLANTSMEN

DAVID FAIRCHILD

PLANTSMEN are born, and are seldom or never made by education. A man may be a good botanist and not be a good plantsman, for a botanist's interests lie in the names of the plants or their morphology or anatomy or cytology or in their chemical constitution. He may be an expert botanist and yet in the country, surrounded by trees and fields of wild plants, be unable to tell one species from another. A plantsman, on the other hand, loves plants for their own sake and, as he wanders through life, forms the habit of knowing the trees and shrubs and weeds growing about him and is not satisfied if he cannot identify at least the families to which they belong. This love goes deeper still if he is a true plantsman. It makes him unhappy if he is not growing plants himself and watching them develop.

The careers of two of the world's true plantsmen have just closed, and in such dramatic fashion that the cable despatches regarding their deaths have been published throughout America: Frank N. Meyer, of Amsterdam, Holland, and Aaron Aaronsohn, of Haifa, Palestine—the one while descending the Yangtze River, the other in a fall in an aeroplane off the north coast of France.

By that strange attraction which brings congenial people together, Frank N. Meyer, for years the assistant to Hugo de Vries in Amsterdam, drifted into the organization of the Office of Foreign Seed and Plant Introduction, became an American citizen and an agricultural explorer, and Aaron Aaronsohn, Director of the first American agricultural experiment station on the shores of the Mediterranean, became a foreign collaborator of the same office.

Through these coincidences I came to know intimately these two remarkable men, whose work in the discovery of new plants for the use of plant breeders has already become history.

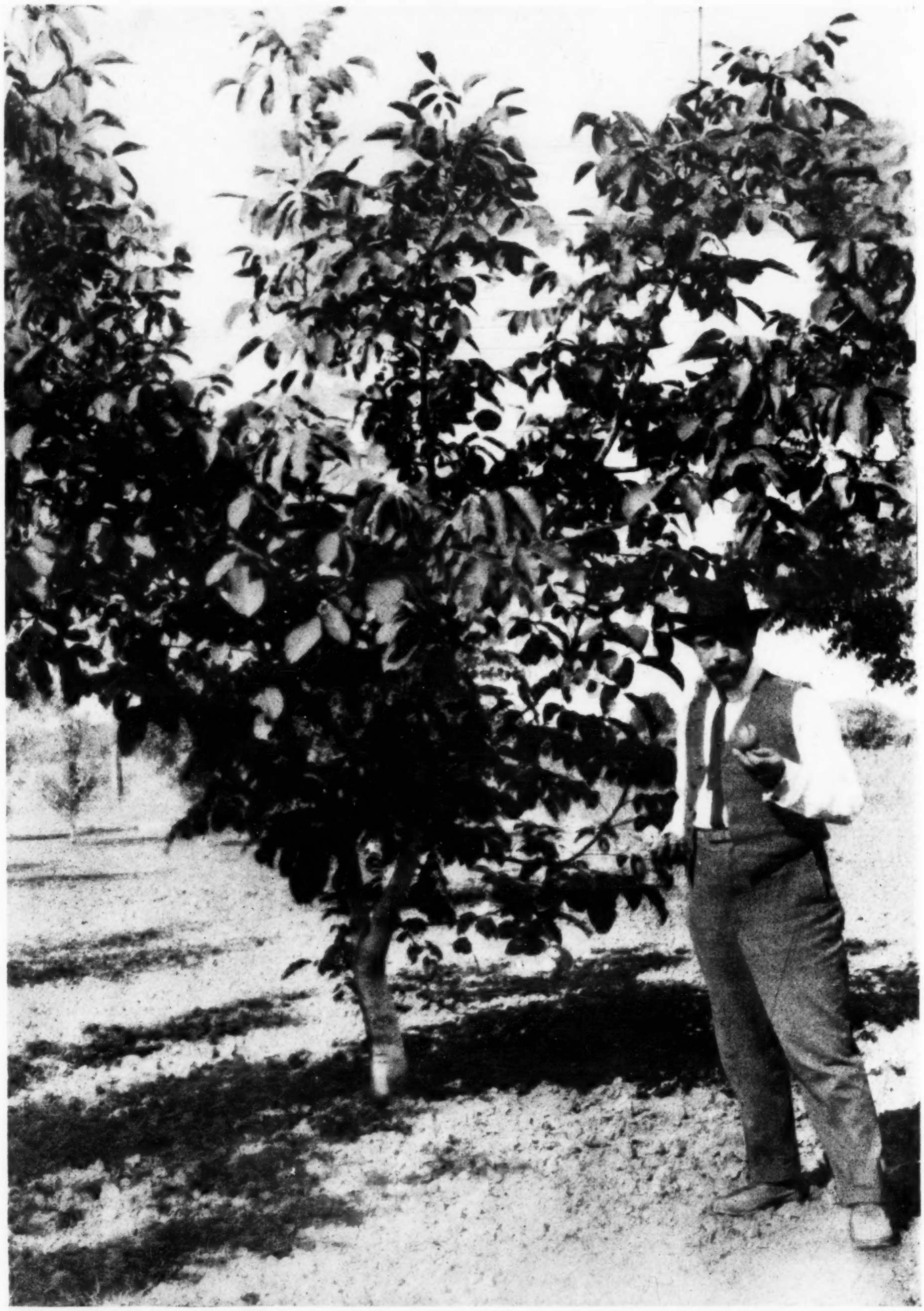
For nine years Frank Meyer wandered on foot along the narrow pathways of China, gathering the plants which he believed would grow in America. As I write of him here, his hardy yellow rose, *Rosa xanthina*, peers in upon me through my study window, and up in the border his scarlet lily is in bud, while the perfume of his lilac has barely passed away. His white-barked pine is dusting its pollen into the air, his Euonymous and his hardy bamboo are growing at the corners of the house, and his dry-land elm with its delicate branches shades the entrance. So much of China has he successfully transplanted to this country.

Meyer's memory of the forms of plants as they appeared in the open was remarkable. In this lay his great power. He could keep in his mind the characters of thousands of plants—many of which he had only once seen—and this enabled him to recognize at once any which were strangers to his experience. It is this form memory, inborn certainly, but trained by years of solitude in forests and by long tramps through the fields looking for flowers, that proves such an invaluable asset in the profession of an agricultural explorer. This, combined with the enthusiasm of a boy to whom everything is new, made Meyer unique as a hunter of plants.

The plant breeders of America and of all countries where the problems are similar will benefit by Meyer's explorations in many ways, for he had the tastes of a plant breeder and expected, when his traveling was done, to settle down in a plant-breeding garden somewhere in the Rocky Mountain region, where a high altitude and cool weather would enable him to carry on breeding work

PIONEER AGRICULTURAL WORK IN THE ORIENT

The first agricultural experiment station to be developed along American lines in the Orient was started at Haifa, Palestine, by Aaron Aaronsohn. Its progress, which was being watched with interest by plantsmen throughout the world, was interrupted by the world war, but Dr. Aaronsohn, who is here seen examining one of his irrigation ditches in company with his secretary and his foreman, intended to resume experimental work the moment peace was restored. Just at the moment when it seemed possible to do this, and he was making plans to that end, he met his death in the fall of an airplane.



FRANK N. MEYER, AGRICULTURAL EXPLORER

The plant explorer's reward comes from seeing the fruits and flowers which he has gathered in remote corners of the earth growing successfully in his own country. Fortunately, Frank N. Meyer lived to see many of his own introductions established in the United States. The one which he is here examining in the Plant Introduction Station at Chico, California, is a walnut which he sent from northern China.

throughout the summer. His notes, published in the "Inventories of Seeds and Plants Imported," of the Office of Foreign Seed and Plant Introduction, are full of suggestions to plant breeders, and, luckily, his suggestions are backed up by living material which will make it easy to provide the breeders with many of the plants which he describes.

Meyer collected a very wide range of plants. He collected wild alfalfas in the Caucasus, Chinese Turkestan and Siberia; sorghums and Chinese pears in Manchuria; wild peaches and almonds in the Kansu Province; chestnuts east of Pekin; persimmons in the Ming Tombs Valley; wild conifers in the Wu Tai Shan; citrus fruits on the Upper Yangtze; bamboos and strawberry trees (the Yang mae) south of Shanghai; jujubes and the pound peach in the Shantung Province; dwarf almonds, dwarf cherries and apricots and large fruited oleasters in Russian Turkestan; desert poplars and tamarisks, wheats and barleys in the desert region of Chinese Turkestan; wild apples and apricots in the Tisu Shan range which divides Siberia and Chinese Turkestan; large fruited black currants from the Yakutsk Province of Siberia.

I doubt if any man has traveled more miles on foot than Meyer did, in search of his plants. He was attacked by ruffians in Harbin; his life was threatened by Chinese soldiers in Kansu, who stood him up against the wall. He spent months in the uncomfortable inns and was quartered in temples and other strange places in China. He ran the gauntlet of suspicious Russian officials on the border between Siberia and China at a time when the relations between these two countries were strained. He had the distinction of having his photographs of deforestation in China used by President Roosevelt in his message to Congress. Shut in by the Chinese revolution in Ichang for many months, his health, which had begun to feel the effects of lonely travel, broke down and, when finally he succeeded in escaping, the strain appears to have been too much. He disappeared from the river steamer in the middle of the night of June 2, 1918, and his body was found several days later by the American consul at the little town of Wuhu, where the Chinese, who had found it in the river, had temporarily buried it, and from which place it was taken to Shanghai, where it now rests.

Aaron Aaronsohn began his life on horseback, so to speak. On his Arab steed he traversed the Jordan and climbed the slopes of Mount Hermon in the Holy Land. Educated as a protégé of Baron Rothschild, in Grignon, France, and as a friend of the African explorer Schweinfurth in Berlin, he early became interested in the wild plants of Palestine and made collections of the wild forms. Urged on by his professors in Bonn and Munich to discover the origin of the cultivated wheat plant, he found on the slopes of Mount Hermon a truly wild wheat, which has been subsequently named by O. F. Cook in honor of the discovery, *Triticum hermoni*. Because of the fact that certain crosses of cultivated wheats revert to it in character, and from the fact of its undoubted wild character, it appears in all probability to be one of the progenitors of wheat, the greatest cereal of civilized man, rather than a cultivated form gone wild.

Coming to America to study American conditions, Aaronsohn interested the Department of Agriculture in his Palestine researches, and through the friends he made here and guided by the advice of department men, he established the first agricultural experiment station along American lines to be started outside the confines of the United States.

After four years of organization work, the war storm broke over the institution, wrecked it completely and forced Aaronsohn to flee. His life, which had been spectacular enough during peace times, became infinitely more exciting, and the story of his escape through Austria and Germany into Denmark on the plea that if he could only get the advice of Johannson of Copenhagen or Nilsson of Svalöf, Sweden, he could produce by a selection a variety of sesame which would produce increased

amounts of this oil-producing seed and relieve the oil shortage, seemed as he told it to me like a chapter out of the Arabian Nights. He told me how he, by cablegrams to America, got the English Consulate to give him papers to England, and how he threw these away in order that he might be arrested as a spy on arrival at Folkstone and so conceal from the Turks the fact that he had deserted and thus save his own family from massacre, only to hear later of their murder; how he showed General Allenby the water-bearing strata where artesian wells should be sunk in Palestine and aided him in placing his artillery so as to produce the greatest effect on the cliffs and fortifications he was attacking; how he saw Balfour and was of influence in getting him to make his promise of "Palestine as a home land for the Jews"; of his work in Bulgaria to pull her out of the war—all these war activities of Aaronsohn's will doubtless find their place in the histories now being written of the Great Struggle.

That, just as the war clouds are lifting and he was making plans to put in new crop plants and start a new agriculture on a quarter of a million acres of land in Palestine, and was hurrying through the air from London to Paris, he and his pilot should instantly drop through the fog into the sea and be lost, seems too hard to believe, for we had come to feel that there on the eastern, Oriental end of the Mediterranean there was coming into existence a type of experimental agriculture which would be epoch-making and that Aaronsohn was the man destined to bring this about. When the war broke, a beginning in plant exchange had been made, and the plant breeders of America will find in Aaronsohn's bulletin on his agricultural explorations in Palestine a large number of valuable suggestions regarding the possibilities of new dry-land cereals, stocks for the jujubes, oil-producing grains, the carob fodder tree, and cover crops for citrus orchards. The date growers in the Salt River Valley have grown some of the date palms which Aaronsohn got for them in Egypt, and the desert hawthorn which he sent in is being tested as a stock for early pears.

The plans of these two remarkable men, the myriads of observations made, the knowledge gained from their reflections in solitude—all are lost to the world at a time when it can ill afford to lose such things and just as their discoveries were beginning to need their guidance.

May their lives encourage, somewhere in the world, young men starting out to live, to find their careers in the field of plant breeding and plant exploration.

Corrections in an Article on Poultry Breeding

Typographical errors unfortunately appeared in the article by Professor Lippincott, "The Breed in Poultry and Pure Breeding," which appeared in the February number, 1919. These are of a technical nature and require correction in order to make the article intelligible. Page 77, eleven lines from the bottom, P=white (no pigment) should read p=white (no pigment). Same page, two lines from bottom; b=indicates, etc., should read *b*=indicates, etc. Page 78, twelve lines from the bottom, for pp (Re)(rE) Bb read pp (Re) (rE) B*b*. Five lines from the bottom, right-hand column, the same correction should be made. The four lines at the bottom should read:

Offspring (1) Pp (Re)(Re) bB = Barred splashed ♂ ♂
(2) Pp (Re)(rE) bB = Barred blue ♂ ♂
(3) Pp (Re)(Re) b*b* = Non-barred splashed ♀ ♀
(4) Pp (Re)(rE) b*b* = Non-barred blue ♀ ♀
} Equal numbers

AN IMPROVED STRAIN OF WISCONSIN TOBACCO

Connecticut Havana No. 38

JAMES JOHNSON

Wisconsin Experiment Station[1]

THE instances of improved types of tobacco, which are accepted as such commercially, together with data covering a number of years to show where such improvement lies, are sufficiently rare to warrant recording. In 1908 a project for the production of an improved strain of Wisconsin cigar binder tobacco was undertaken, together with a study of certain principles of inheritance involved in crosses and in simple selection with tobacco. Some of the results secured on the latter problem are to be published elsewhere. It is desired here to record only certain aspects of the production of a commercial strain of tobacco.

During the summer of 1908, about 2 acres of Connecticut Havana tobacco were grown at the Experiment Station Farm at Madison, largely for seed purposes, with the idea of introducing this variety widely throughout the tobacco-growing regions of the State. Several hundred pounds of this seed were distributed annually for a period of three years and it met with considerable success. A great many growers, however, reported that although the quality was very satisfactory the yield of this type should be increased. The exact history of the strain of Connecticut Havana which was grown is not definitely known, although at the time of its introduction here it was not far removed from some of the best strains grown under this name in the Connecticut Valley. Repeated comparisons with the widely grown variety of Comstock Spanish of Wisconsin, the Connecticut Havana, and the Zimmer Spanish of Ohio, showed that they were all practically identical and could be all classed in one group as "Havana Seed."

The Connecticut Havana grown for seed in 1908 was presumably "pure-bred" seed. A great deal of attention was paid to roguing out seed plants and selecting and bagging plants for further study during this year. Seed from about thirty-five plants was self-fertilized and the majority of these sowed separately and transplanted into progeny rows in 1909. It became evident from this test that three fairly distinct types of tobacco were present in the progeny plat, but that the progeny from each parent plant was remarkably uniform. The majority of the rows were practically identical and typical Connecticut Havana tobacco, but two rows, viz., Selection No. 26 and Selection No. 27, stood out distinctly from the others. Type 26 differed from the normal type in that the leaves were smaller in number but considerably larger in size, and also were more pendant than those of 27. Further, the stalk was shorter, thicker, and tended to lodge more than the normal type under unfavorable weather conditions. Type 26 also budded slightly earlier and matured earlier than the normal. Type 27, on the other hand, differed from the normal in that it possessed a greater number of leaves, which, on the whole, were somewhat smaller in size than the normal. The leaves were very erect in position. The stalk was a little more slender, somewhat taller than normal, but very strong, and rarely lodged. This strain

[1] Published by permission of the Director.

ONE OF THE PARENTS OF CONNECTICUT HAVANA NO. 38

This tobacco plant represents Strain No. 26, isolated from the Connecticut Havana variety. It is the female parent of Strain No. 38, of which 10,000 acres are now grown commercially in Wisconsin. (Fig. 8.)

budded and matured on an average of a week or ten days later than the normal.

It is assumed that strains 26 and 27 are "mutations" from the normal. The reasons for this assumption lie primarily in the fact that both types differ from any other variety or strain which has previously or since been grown at the station, and hence their occurrence in the Connecticut Havana variety was presumably not due to an intermixture of seed. On the other hand, these strains have now been grown for ten years and during this time have shown no sign of segregation but have remained remarkably uniform as to type.

Both strains 26 and 27 possessed some commercial advantages over the normal, as, for instance, the size of the leaf in 26 and the number of leaves in 27. On the other hand, both possessed points of lesser value than the normal, as, for instance, the reduced number of leaves in 26 and the reduced size of leaves in 27. The quality of the leaf in both seemed to be in no way inferior to that of the normal, although the time of maturity of 27 might, under certain conditions, prove undesirable.

It at once became evident that, if the desirable qualities of the two strains could be combined into one plant, a considerable improvement over the normal might be expected. Accordingly this cross was made along with a large number of other crosses of varieties and strains made during 1909 for the same purpose. The first generation of the cross 26♀ × 27♂ was given in 1910. The cross showed to a very large extent a very desirable intermediate condition of the characters of the two parents, and, in an unexpected manner, produced a plant whose conformation very closely approached the ideal in mind. This type was given the number 38 as a means of readily distinguishing it from the numerous other types grown. The second generation of the cross was grown in small numbers in 1911, but in 1912 and succeeding generations was grown in large numbers, although data were unfortunately not taken upon large numbers in the second generation. Field observations upon a large number of individuals, however, have convinced the writer that there was no marked segregation into types such as occurred when certain varieties of tobacco were crossed and grown under similar conditions. Whether the segregation was of such character that it could not be detected or did not occur at all may be regarded as an open question. The writer is inclined to believe, however, as a result of data and observations on this and other crosses in tobacco, that under certain conditions, where the strains crossed are closely related, "blended" inheritance only may occur, segregation into parent types and intermediates being absent in the second filial generation. Whether this assumption, which has been noted in several variety crosses of tobacco (together with segregation in others) be true or not, it is evident that differences in habit and form of growth and in quality which cannot be detected by careful observation can have no significance from a commercial standpoint, although segregation in characters concerned in quality may occur. No evidence of such segregation has been found, but it is exceedingly important that no segregating strain, no matter how small the variability may be, should be introduced into cultivation. In order to be further certain of this, therefore, the new strain was grown for four years at the station before being distributed to growers and then the distribution was limited to small amounts for experimental purposes. Following fairly extensive distribution in 1916, however, the writer was convinced that the quality was sufficiently uniform to warrant the general recommendation of this seed to Wisconsin growers. The strain has met with considerable favor among the growers from the standpoint of appearance and yield in the field and from the standpoint of the cured product on the part of the buyers. The strain owes its value, not to its being widely different from the best varieties grown in the State, but rather to its general similarity to these varieties combined with the fact that it is a

distinct improvement over them in some respects. Such improvement apparently could not be obtained from the standard variety by simple selection alone, as has been shown by such trials carried on simultaneously.

The chief advantages claimed for Connecticut Havana No. 38 are due to the desirable habit of growth it presents in the field, the leaves being quite erect in position, rendering the necessary field operations more easily accomplished. The leaves are quite uniform in size from top to bottom of the plant and are somewhat broader in proportion to their length than those of the ordinary Havana seed. It possesses on the average one or two more leaves to the plant than does the ordinary Havana, while, at the same time, it has a greater average size of leaf. The quality is not inferior to that of Connecticut Havana seed, at least under Wisconsin conditions. Although no carefully controlled experiment on the relative yield of Connecticut Havana No. 38 and of ordinary Havana seed has been made, it appears from the data secured on leaf number and size that on the average it will yield 7 to 10% more than the ordinary Havana seed.

The data upon which most of these conclusions are based are presented in Table I, covering a period of nine seasons. During each year measurements and counts were made on about twenty-five plants of each type and the averages only are given for the four types 26, 27, 38, and 33, the latter being a product of a continued selection and imbreeding experiment with the ordinary Connecticut Havana type. During two or three years all the data could not be secured, either because of insect injury or, as in 1916, when the data were discarded as unreliable because of the effect of Mosaic disease upon the plants. In other years the root-rot disease due to Thielavia was a factor in reducing size, but it was considered that the influence had the same relative effect on each, since in these types there was no evidence of variation in resistance to the disease. It should be said here that Connecticut Havana No. 38 is no more resistant to the Thielavia root-rot disease than the ordinary Spanish or Havana types of seed, and the grower is likely to be disappointed in low yields obtained on infested soils. This strain preferably should not be grown on Thielavia infested soils. Where the grower expects to plant tobacco on root-rot infested soils or on old tobacco fields, he should obtain a resistant type of seed. Such types are now being developed at the Wisconsin Station in coöperation with the United States Department of Agriculture and may in the future largely displace the susceptible strains of Havana tobacco, provided an equally high quality can be produced in the resistant types.

Referring to Table I, it will be noted that numbers have been gradually added to the designation. Each digit following the original number indicates an added generation of inbreeding so that the number of generations a seed-plant has been inbred can be readily determined. The stalk height refers to the height of a "topped" plant. The height of topping was in some measure determined by judgment from a commercial standpoint in earlier years, but in the later years the plants were topped uniformly with reference to the position of the first "bald" sucker. The leaf number is the number of marketable leaves. The length and breadth of the leaves are the maximum measurement of these dimensions. The "top leaf" is the uppermost leaf of the plant after topping, the "middle leaf" the largest leaf selected at random at about the middle position of the plant and the "bottom leaf," usually the third from the bottom counting in the "sand" leaves. The average leaf length and width is the average of the top, middle, and bottom leaf, and represents quite satisfactorily the average size of the leaves on the plant. The average breadth-index is secured by dividing the average leaf breadth by the average length and multiplying by 100. This figure is a good relative index to the shape of the leaf, *i. e.*, it indicates the width as compared to the length, the greater the number the more nearly

AN IMPROVED WISCONSIN STRAIN OF TOBACCO

"Connecticut Havana No. 38," a strain produced by crossing two strains isolated from the common Connecticut Havana variety. This type of tobacco has been a commercial success, and is a noteworthy example of the practical possibilities of plant breeding. (Fig. 9.)

THE SECOND PARENT OF CONNECTICUT HAVANA NO. 38

This tobacco plant represents Strain No. 27, isolated from the Connecticut Havana variety. It was used as the male or pollen-bearing strain in making the cross which produced the commercially valuable type known as Connecticut Havana No. 38. (Fig. 10.)

TABLE I.—*Data for Leaf Number, Size, and Shape, and for Height of Plants of Connecticut Havana No. 38 in Comparison with Parent Plants and a Normal Strain for Years* 1909–1918

Year	Designation	Average of number of plants measured	Stalk height	Leaf No.	Top leaf		Middle leaf		Bottom leaf		Leaf averages		Average
					Length	Width	Length	Width	Length	Width	Length	Width	Breadth-index
1909	26(P1)	25	30.1	16.1	15.7	7.7	.....	.....	17.8	12.0	16.7	9.8	58.8
	27(P2)	25	31.3	18.2	12.0	5.2	.....	.....	15.7	10.9	13.8	8.0	58.0
	33	25	32.0	16.8	14.2	6.8	.....	.....	16.2	10.8	15.2	8.8	57.8
1910	38(F1)	20	31.1	18.6	12.4	7.4	.....	.....	17.3	12.4	14.8	9.9	66.0
1911	263	17	31.0	15.1	15.3	8.7	23.1	14.3	19.0	12.8	19.1	11.9	62.3
	274	18	36.3	19.1	16.1	7.6	21.4	11.9	17.7	11.6	18.4	10.3	55.9
	381(F2)	46	29.2	16.8	15.5	7.8	21.2	11.6	17.9	11.9	18.2	10.4	57.1
	333	15	27.8	15.9	15.0	7.4	20.0	10.7	15.4	11.1	16.8	9.7	57.7
1912	26302	15	28.0	14.2	19.9	10.0	25.6	13.2	19.0	11.3	21.5	11.5	53.4
	27402	25	35.0	17.9	18.3	8.3	23.0	12.6	18.0	10.8	19.8	10.6	53.5
	383(F2)	25	32.8	17.1	20.3	9.2	24.9	12.9	19.2	11.1	21.5	11.1	51.6
	33304	25	31.2	16.1	18.7	8.4	23.7	12.3	18.3	10.0	20.2	10.2	50.5
1913	26	25	31.5	15.1	21.8	12.0	26.0	14.4	18.0	11.5	21.9	12.6	57.1
	274021	25	32.9	17.5	20.6	9.7	24.7	13.1	17.0	11.0	20.8	11.3	54.3
	38(F1)	50	33.8	17.0	20.5	10.3	25.6	13.2	17.6	10.9	21.2	11.5	54.2
	333042	15	33.3	15.4	20.8	10.4	25.4	13.7	17.2	11.2	21.1	11.8	55.9
1914	2740211	25	29.9	18.0	15.5	7.5	20.8	10.6	15.9	10.7	17.4	9.6	55.1
	387(F2)	25	30.0	16.0	16.0	7.7	22.1	11.2	15.5	10.6	17.9	9.8	54.2
	3330422	25	29.0	15.4	15.0	7.4	21.3	10.8	15.6	9.6	17.3	9.3	53.7
1915	26301	25	24.4	12.9	17.0	8.7	24.0	12.6	18.9	11.1	20.0	10.8	54.0
	27402112	25	25.8	16.5	16.7	7.4	22.1	10.4	16.8	9.2	18.5	9.0	48.6
	386(F2)	25	23.2	16.2	18.6	8.3	22.9	11.6	18.4	10.9	20.0	10.3	51.5
	33304222	25	27.7	14.9	15.8	7.5	21.7	10.8	16.8	8.4	18.1	8.9	49.1
1917	263016	25	29.0	12.2	17.9	9.6	24.8	13.6	17.3	10.5	20.0	11.2	56.0
	274021121	25	37.4	18.0	14.1	6.7	21.8	11.5	16.5	9.5	17.5	9.2	52.5
	3862(F3)	25	34.7	15.8	16.4	9.0	24.7	14.1	17.2	11.1	19.4	11.4	58.7
	33304221	25	31.4	13.8	17.1	8.9	23.9	12.7	16.2	10.0	19.1	10.5	54.9
1918	2630161	25	31.5	13.8	19.8	9.4	27.4	14.2	16.9	10.1	21.4	11.6	54.2
	2740211214	25	39.2	19.2	14.7	6.1	24.6	12.4	14.4	8.5	17.9	9.2	51.4
	38621(F4)	25	35.5	17.3	18.2	8.6	26.9	14.2	15.6	9.6	20.2	10.8	53.4
	3330422214	25	32.0	16.0	17.1	8.0	24.1	12.8	13.0	7.2	18.1	9.3	51.3

"round" the leaf. From these data it will be seen, by comparing the averages of No. 38 with those of No. 33 (the ordinary Connecticut Havana), that the number of leaves is on the whole greater and the average leaf size somewhat larger in the Connecticut Havana 38. The leaf shape is also considerably better under good growing conditions, though this character is less pronounced than other characters under unfavorable growing conditions. The more significant facts are summarized as averages of eight years in Table II.

The value of this strain is shown by the fact that in addition to the seed distributed by the station, at least four growers are producing the seed for commercial purposes in one instance on an extensive scale. *It is estimated that at least ten thousand acres of this type will be grown in Wisconsin during* 1919, *out of a total acreage of forty to forty-five thousand. This is, then, a case of a new strain of tobacco developed as a result of experimental trial which has been accepted and grown commercially.* There have been perhaps a number of such failures in tobacco culture leading to the popular opinion among growers and dealers that tobacco cannot or should not be "bred up," but that we should strive to maintain the "original" as closely as practicable, separating our seed by as few generations as possible from the original stock of seed upon which the success of the tobacco industry was founded. The fallacy of such an argument is evident from an agricultural standpoint, although at the same time one cannot argue too strongly against the promiscuous introduction of new strains of tobacco without having first repeatedly tested them out under the commercial conditions for which they are intended. Much progress can be made along the line of improving tobacco by breeding or selection, but it should not be attempted by anyone who is not willing to devote at least eight to ten years of time and work to accomplish it.

TABLE II.—*Summarized Data of Most Significant Characters of Connecticut Havana No. 38, Together with Parent and Normal Strains. Averages of eight years.*

Strain	Leaf number	Average of top, middle, and bottom leaves		Breadth index of leaf
		Length, inches	Width, inches	
No. 26	14.2	20.0	11.3	56.5
No. 27	18.0	18.0	9.6	53.6
No. 38	16.9	19.1	10.6	55.8
No. 33	15.5	18.2	9.8	53.8

Notice to Readers

Attention is invited to the footnote on the title page, in which it is stated that the JOURNAL is published monthly with the exception of July, August and September. The omission of these three numbers was made necessary last year by war conditions, and the fact that the entire editorial staff had entered the military service and left the magazine in the hands of volunteer workers, making the issuance of the summer numbers impracticable.

It is considered advisable to pursue the same policy this year, but the editorial staff has recently been reorganized, and with the appearance of the October issue it is expected that the JOURNAL will return to its pre-war basis of twelve issues a year.

The JOURNAL relies upon members of the American Genetic Association for interesting material upon subjects falling within its scope. Members are cordially invited to submit articles suitable for publication, especially those which are illustrated with striking or unusual photographs. It is a particular policy of this publication to interpret the discoveries of modern science by means of photographs.